THURSDAY, MARCH 5, 1891.

AN ANTI-DARWINIAN CONTRIBUTION.

The Darwinian Theory of the Origin of Species. By Francis P. Pascoe, F.L.S., &c., ex-President of the Entomological Society. (London: Gurney and Jackson, 1890.)

TO avoid any misconception to which the somewhat ambiguous title of this work may give rise, let it be at once stated that this is not an exposition of the Darwinian theory; neither is it a contribution to that theory in the constructive sense, nor does it attempt to deal with the subject in any other than a destructive sense. The author has, in fact, played the same part in the present little book of some 113 pages that he played some years ago in a similar production which is now out of print, and which bore the title "Notes on Natural Selection and the Origin of Species." Mr. Pascoe has hardly placed himself in the position of "counsel for the opposition," inasmuch as he can scarcely be said to plead anywhere against the Darwinian theory; he acts rather as solicitor for the opposition, and by virtue of his special knowledge of certain groups of insects and his general knowledge of other groups of animals, he has been enabled to collate a large number of difficulties and objections which have occurred to himself and to other naturalists. It is hardly necessary to say that many of the most weighty objections raised by the author have been culled from the writings of Darwin himself. The result has been the production of a brief which is valuable as coming from a systematist of recognized authority, and which the counsel for the opposition may and no doubt will make use of.

In so far as the author quotes from other writers, it is difficult to ascertain how far he has himself grasped the Darwinian principles. Where he does appear in his own personality, he leaves some doubt as to whether he really does understand these principles. For example, there is a passage fronting the title-page which is not given as a quotation, and which we may therefore suppose to be the author's own, and which commences with the statement:—

"Natural selection is assumed to depend on a power in every organism—past and present—intently watching every variation, rigidly destroying any in the least degree injurious, and picking out with unerring skill all that in the future, by gradual accumulations, give a better chance in the struggle for life."

It is difficult to see how natural selection can depend upon a power in an organism either present or past; in fact, selection as understood by most Darwinians is an operation from without, *i.e.* external to the organism; and if the author means to imply that the selection is due to an internal agency, then he has failed to understand the spirit of Darwinism. But the whole passage is so ambiguous that the most favourable view that can be taken of it is that when Mr. Pascoe's counsel comes to consider his brief he may be told that the "power in the organism" is used as a metaphorical expression only.

In a few pages of introduction we have a series of quotations, or rather fragments of quotations, from Herbert Spencer, Agassiz, Cope, Wallace, Huxley, Flower, Mivart, and Tyndall. These are strung together without any connecting argument, but are presented in a form which is, to say the least, unfair to the authors in question. They are the odds and ends of various passages which would in most cases bear a quite different construction from that which the author intends his readers to put upon them. He quotes from Darwin to the effect that "with all beings there must be much fortuitous destruction, which can have little or no influence on the course of natural selection." Then he adds:—

"It cannot be contended, for instance, that out of the ten million ova of a codfish the few that attain maturity owe their escape to something not possessed by those that perish."

Certainly not; all that Darwinians would contend for is that out of the millions of codfish whose ova do reach maturity, there would be quite a sufficient amount of material for natural selection to work upon. For this agency can obviously deal only with the organism which is already in existence, and if by fortuitous destruction a very large proportion of the ova are destroyed, that percentage is *hors de combat* so far as natural selection is concerned. Moreover, the production of large numbers of ova shows that natural selection has been at work in the direction of producing an increased fertility. This "objection" does not, therefore, appear to be of a very serious character.

After the introduction the real business of the book begins with a brief historical sketch. The statement of the Darwinian theory is composed chiefly of quotations, and in so far as these are Darwin's own words, they are fairly presented. But when the author offers his own summing up, we again meet with that indistinctness of expression which is one of his most unsatisfactory characteristics:—

"Natural selection, then, is assumed to be a power acting by gradually accumulating small modifications, which, in the future, when increased by inherited modifications, is to the 'advantage of each being.'"

Mr. Pascoe's counsel must be left to put this into form. Sexual selection is, of course, not received by the author, but his objections to this part of the theory are very meagre, and he does not refer to any of the recent contributions to the subject, such, *e.g.*, as the masterly series of observations on spiders by George and Elizabeth Peckham. It can hardly be supposed that a writer possessed of such an extensive knowledge of the literature of the Arthropoda, should not be aware of the existence of this work. In connection with the subject of use and disuse, there is a discussion of the question of the wingless beetles of Madeira, which is of importance, because it deals with a group of insects upon which the author is entitled to pronounce an opinion as an expert. Darwin's explanation of this fact is familiar to all students of the "Origin of Species." The "objections" brought against this explanation are thus stated:—

"Mr. Wallace says that the same species, wingless in Madeira, are winged on the Continent. It may be so. Some of our Hemiptera—*Nabis*, *Pithanus*, *Pyrrhocoris*, &c.—ordinarily wingless, are sometimes found in hot summers to have well-developed wings. But the beetles

of Madeira belong, without exception, to European forms, and in Europe few species are habitual fliers, while wingless, or nearly wingless, species are found far inland, in all parts of the world, belonging principally to the same families as those in Madeira (Curculionidæ, Tenebrionidæ, Carabidæ, Lamiidæ, &c.). It is difficult always to determine, without injury to the insect, what may be a wingless species, but in these four families alone it is safe to say that there are a few thousands. In many cases the elytra are soldered together, and have even grown to the sides of the thorax. In some Lamiidæ allied to *Dorcadion*, and probably in some others, there are vestiges of wings. But how is the union of the elytra to be explained? . . . There is a tendency to be wingless in all the insect orders, either in one or both sexes. Neuters, unlike males or females, are always wingless. Barren females are only neuters from want of food."

That is to say, that because winglessness occurs in other insects through other causes than those which obtain in Madeira, therefore this is an "objection" to the wind-selection theory of Darwin. Since Mr. Pascoe quotes Huxley to the effect that "in science scepticism is a duty," I may be permitted to express my scepticism as to whether the author has really grasped the Darwinian explanation. I must confess that to me the reasoning appears to be of the nature of a *non sequitur*, but I have given the extract in full in order that readers may form their own opinion. It is typical of the whole spirit of the "objections" which the author has marshalled against the Darwinians. In the first place, the above statement does not fairly represent Mr. Wallace's views. He says only that *some of* the wingless Madeiran species are winged on the Continent. "In other cases the wingless Madeira species are distinct from, but closely allied to, winged species of Europe" ("Darwinism," p. 105). But the fact that Darwin sought to explain is not that there occur wingless beetles in Madeira, but that such a large number (over 34 per cent.) out of the whole Coleopterous population should be thus deprived of the means of flight. Moreover, the confirmation which this explanation receives from the insects of Kerguelen, which belong to three different orders, and which are *all* wingless, is not alluded to as it should have been in fairness to the argument. Even the "union of the elytra" may disappear as an objection on further consideration, for if, through wind-selection, *or any other cause*, flight became unnecessary or dangerous, it is quite conceivable that the possession of loose elytra would become a source of positive danger to beetles which bury or live among loose stones and the crevices of bark, and natural selection would in such cases bring about the character in question on account of the superior protection from bodily injury which would be thus furnished.

The next writers whom the author puts into his anti-Darwinian witness-box are Dr. Romanes and Prof. Mivart, the latter of whom he considers to be the "most damaging" opponent. Then we have a whole array of Darwin's own difficulties, which are familiar to all who have read the later editions of the "Origin of Species." In his anxiety to pin Darwinians down to the literal statements of their master, Mr. Pascoe has not done justice to, or has wilfully ignored, the later developments of the theory. It is true that Darwin admitted that, unless "many individuals were similarly and simultaneously modified 'rarely single variations could be perpetuated.' . . . Variation, however useful, if confined to one individual, would be 'generally lost by subsequent intercrossing with ordinary individuals.'"

But ideas on this head have become broadened to an extent that the author does not seem to be aware of. The whole number of individuals composing any species may at any period of its existence be divided into two portions presenting variations above and below a line of mean variability. Of these portions one must possess characters more or less advantageous so far as concerns the external conditions, and the other portion must possess characters more or less disadvantageous with respect to those same conditions. On the first portion—which may be considered above the mean line—natural selection will act as a preserving agent; on the second portion—below the mean line—natural selection will act as a destructive agent. That is to say, that the whole number of individuals surviving in the struggle for life is supplied from the portion *above* the mean line of variability. The proportion of individuals of this upper portion which survive will depend upon two factors, the intensity of the action of selection for the time being, and upon the range of variability of the species, *i.e.* the height of the extreme variations above the mean line. Mr. Pascoe occasionally gives extracts from Wallace's "Darwinism," but he cannot have properly digested the third chapter of that work, although he quotes from it, if he still considers the isolated fragments of Darwin's writings which he gives above in the light of "objections." It is obvious that many individuals *are* similarly and simultaneously modified, and since the action of selection in the conservative sense is exerted upon individuals above the mean, while the loss by destruction falls upon those which are below the mean, it follows that in the next generation the line of mean variability will be raised, *i.e.* the species will have come into closer harmony with the external conditions, and so on in successive generations till equilibrium is reached.

Among the objections for which the author makes Dr. Romanes responsible is the well-known one about the giraffe:—

"On the converting 'an ordinary hoofed quadruped' into a giraffe, Mr. Romanes observes: 'Thousands and thousands of changes will be necessary.' . . . 'The tapering down of the hind-quarters would be useless without a tapering up of the fore-quarters.' The chances of such changes are 'infinity to one' against the association of so many changes happening to arise by way of merely fortuitous variation, and these variations occurring by mere accident."

I cannot say how far this passage represents Dr. Romanes's views. The latter portion appears to contain a distinct pleonasm, but this is a point of detail, arising perhaps from the author having torn the passage from its context and then dissecting it. But surely it is not essential to the Darwinian explanation of the form of the giraffe that there should have been any "tapering down" at all. The particular advantage which the form of the animal confers upon it is its *height;* the neck has been elongated and the fore-quarters raised for this purpose, and, as far as I know, neither Darwin nor any of his adherents have asked us to believe that the hind-quarters have been lowered. The other difficulty presented in the above passage is one which is very frequently urged, viz. the chances against concurrent favourable variations in

complex and co-ordinated structures. To many impartial critics, however, this class of difficulty will not appear so formidable when it is remembered that the complex co-ordinations which are now witnessed are the final results of long series of modifications superimposed from generation to generation through long periods of time. We are not called upon to believe that co-ordinated characters were developed at once by the occurrence of a sufficient number of individual variations having the necessary co-ordination of distinct variations. Mere elongation of neck would be an advantage to an animal occasionally driven to browse upon the foliage of trees. The most rabid anti-Darwinian will not deny that animals do vary in this character. But if an elongated neck is an advantage, natural selection would seize upon it and perpetuate it in the species presenting the necessary variations in this direction. Then, superimposed upon this character would be other modifications, say strength of neck-muscle, which would give its possessors an advantage in reaching up to their food. Natural selection would, in such a case, be acting upon two distinct characters—length of neck and strength of muscle—which are now co-ordinated, inasmuch as they have both been developed for a common purpose. But it is not necessary to suppose that the variations in these two characters have always been "concomitant"; there is no reason for believing that the individuals with the longest necks were necessarily those with the strongest muscles. All that is claimed is, that among the individuals with the longest necks there would be variability in the strength of the neck-muscles, and that muscular strength has thus been superadded little by little to length of neck through successive generations, until the co-ordination which we now observe has been reached. Whole classes of difficulties in the way of accepting incipient "concomitant variations," which occur throughout the present work and others of a like character, will, I venture to think, disappear, or at any rate become much diminished, if looked at from this point of view.

About one-half of the whole book is devoted to objections of this and other kinds, but the reader, whether Darwinian or not, will be struck by the total want of method with which the facts are marshalled. This is certainly a surprising defect in a writer of such acknowledged skill as a systematist. Large numbers of the paragraphs are bare statements of facts, and their bearing upon the questions at issue is often so obscure that it is impossible to decide whether the author is writing in favour of Darwinism or against it. The first indication of methodical treatment appears about the middle of the book, where begins a short synopsis of the Invertebrata, which occupies from thirty to forty pages, and of which the object is thus explained:—

> "The invertebrate classes are so little noticed by writers on the origin of species, that a short sketch of some of their characters and peculiarities may be useful to show their classification and affinities, and to give some idea of their often extremely exceptional forms and modes of reproduction. It will bring into special notice the statements already given in general, and illustrate still further the difficulties of the theory."

This synopsis is certainly useful as far as it goes, but it contains only the ordinary zoological information (very much compressed) to be found in any modern text-book, and therefore might well have been dispensed with in a special work like the present where compactness appears to have been aimed at. If the author had only devoted the same amount of space to the detailed discussion of some special classes of difficulties he would have done his own cause more service. To say that "the invertebrate classes are so little noticed by writers on the origin of species" is a distinct injustice to such writers as Wallace, Bates, Fritz Müller, Haeckel, Lubbock, Weismann, Ray Lankester, and Poulton, all of whom have based their contributions to the theory in question, either partially or wholly, on work done in connection with the invertebrates.

After the summary of the characters of the Invertebrata, Mr. Pascoe introduces the subject of colour as a means of protection. He objects (apparently) to natural selection as the effective agency in this case, quoting Mr. Butler to the effect "that no insect in any shape was ever refused by all birds; what one bird refused another would eat." This objection is based on a misapprehension; no Darwinian has ever asserted that protective colouring is *absolute*, and that the concealment is so perfect that the species thus protected are altogether free from persecution. The utmost that is claimed is a relative advantage. In a similar way Bates's theory of mimicry is lightly disposed of because "it does not appear that butterflies are anywhere the food of birds." There is, however, much evidence to the contrary, and the theory of mimicry, moreover, does not require us to believe that birds are the only foes of butterflies. "Another butterfly (*Hypolimnas bolina*) apparently not mimetic," is a quite erroneous statement, as the female presents one of the most remarkable instances of mimicry known. The leaf-butterfly (*Kallima*) is described as "one of the most perfect cases of mimicry"; but this is rather a case of protective resemblance (Poulton's "procryptic" group). To say of mimicry that, "after all, the majority of such resemblances are superficial," is no objection, but would be conceded by the most advanced advocate of Darwinism. The author records a very interesting case of a Queensland beetle (*Saragus*) which is covered with a waxy secretion which is identical in appearance with a fungus (*Isaria*). The bent of the writer's mind will be gathered from the statement that the fact that the beetle is found on the trees on which the fungus grows is regarded as "the oddest thing" (!). Then he shuts his eyes to the only reasonable explanation, because the beetle belongs to a family which have a hard exoskeleton, and "none of which are likely to be touched by birds." The reasoning is inconclusive, because hardness of covering is not always a protection, and birds are not the only beetle destroyers. With respect to this whole question of colour adaptation, Mr. Pascoe makes a most astonishing statement: "My experience leads me to the conclusion that, as a rule, animals think very little of concealment." If this is his experience, it is certainly at variance with that of every naturalist who has observed animals among their natural surroundings.

The concluding portions deal with Weismann's theory of heredity, instincts, geographical distribution, and other miscellaneous subjects, which are all treated of in the

fragmentary way that prevails throughout. But in spite of any hostile criticism which the work may call forth, it must be recognized that the author has a distinct claim to make himself heard. He is an authority on certain groups of Coleoptera, and is a veteran among systematists who had won his spurs before his present critic knew a beetle from a moth. To attempt to answer all the objections and difficulties which he has accumulated would necessitate a volume. With respect to these objections, some are old, and have already been answered; others are trivial, and require no answer; others answer themselves; others are strained; others arise from misapprehension, as I have endeavoured to show; while others remain as real difficulties, for the solution of which we may have to wait patiently for years. It must not be forgotten, however, that difficulties are not necessarily disproofs. Astronomers tell us that there are many difficulties connected with the motions of the heavenly bodies, but I am not aware that these difficulties have shaken their faith in the theory of gravitation. The man of science accepts that theory, although he might find it impossible to explain why a particular pebble found its way to the top of a particular hill.

In justice to Mr. Pascoe, from whom I have long ago "agreed to differ" on these questions, it must be pointed out that he is, or at least appears to be, an evolutionist. He says so more than once in this work:—"The objections now are confined to natural selection. No objection can be advanced against the theory of descent. A separate creation for each species would admit of no blood-relationship" (p. 107). "We are grateful to Mr. Darwin for having freed science from the bonds of the old theology" (p. 111). It is with the Darwinian form of evolution that he wages war, and if I have availed myself of the opportunity afforded by the Editor of NATURE for discussing the contents of this volume at greater length than its bulk might appear to demand, it is because the author is the representative of a large class of systematic entomologists in this country, who hold similar views, and who, by their constant study of and search for minute characters upon which to found "species," have become biased in judgment with respect to the broader problems of modern biology. Writers of this school are in the habit of forcing difficulties upon natural selection which that theory has never professed to deal with. The Darwinian doctrine will not collapse under this last attack, and however much we may differ from the author, it cannot be denied that, as a stimulus to further research, such compilations as that which Mr. Pascoe has produced are distinctly useful.

R. MELDOLA.

THE HISTOLOGY AND PHYSIOLOGY OF GRANITE.

Contributions to a Knowledge of the Granites of Leinster. By W. J. Sollas, LL.D., D.Sc., F.R.S., Professor of Geology and Mineralogy, University of Dublin. Transactions of the Royal Irish Academy, Vol. XXIX., Part XIV., January 1891. (Dublin.)

THE convenient system of the Royal Irish Academy, by which each paper in their Transactions constitutes a separate part, makes this quarto memoir of nearly a hundred pages virtually an independent work. As such it should be possessed by all engaged in serious petrographic research; while to workers in other branches of geology, it will give a clear idea of the patient methods and precise observations by which the history of rock-masses is gradually being brought to light. The merchant and the *connoisseur* of ornamental stones will find little in these pages concerning the broad features of granite, the joints that so often limit the exposed masses, the modes of disintegration, or even the life-history of the Leinster mountain-chain; while the lover of scenery will miss with regret the handsome plates, connecting surface features with geological structure, which adorned the Transactions of Societies at the commencement of the present century. But we would refer such readers to the limitations and restraint shown in the title of the present paper. It is the work of one who appreciates the labours of his predecessors; yet, despite the bibliography which Prof. Sollas has here drawn up, we soon become aware that our knowledge of the familiar rock, granite, is greatly in need of these "contributions." The detailed chemical and numerical observations of Dr. S. Haughton, so freely quoted from by geological writers during the last thirty years, have clearly borne good fruit in Dublin on their natural soil.

The minerals of the Leinster granite are considered in this paper individually. In obtaining the specific gravity of isolated grains, the author ingeniously employs a "diffusion-column," composed of heavy liquid, the density of which increases regularly from above downwards (see NATURE, February 26, p. 404). It is found that a column prepared by pouring a less dense upon a heavier solution acquires by diffusion the necessary regularity after standing for some six or seven hours. Minerals of known density are introduced as index-marks, and the density of any layer of the liquid between those at which they float can be read off against a graduated mirror. The convenience of this method, as compared with the ordinary plan of uniformly diluting a sample of liquid for the examination of each separate mineral, will be apparent to all who have worked at the determination of rock-constituents; the purity, moreover, of a group of grains, which have been isolated by the ordinary method, may be ensured by transferring them to the diffusion-column, when some may be found to float above and some below the horizon of the pure material.

We notice that Prof. Sollas uses in his investigations the solution of iodides of mercury and potassium: without being contentious over matters of priority, we must protest against his styling this the Thoulet solution, since its inventor, Mr. Sonstadt, was the first, not only to employ it for the determination of specific gravities, but to suggest its utility in processes of isolation.

The author, when dealing with the biotite of the Aughrim granite, discusses in some detail the constitution of the micas, and graphically represents their molecules in the form of sexradiate rings. Now that the forms of molecules are playing a prominent part in crystallographic considerations, chemists will be attracted by these symmetrical diagrams, which are, of course, mainly speculative. Prof. Sollas himself regards them merely as suggestive; but we can imagine some fascinated student, gazing on graphic formulæ which so skilfully

adapt themselves to the reactions of the mineral, and feeling himself at last in touch with the affinities of the impenetrable atom. Let such consider and beware.

The description given of the contact-phenomena (p. 480) has at present an especial interest, and the author freely uses the term "schist" for the distinctly foliated rocks which are common along the flanks of the great chain, although such rocks are undoubtedly of Ordovician age. This is mere justice to the structures developed, and an encouragement to petrologists in the field, who are apt to resent a terminology based in any way upon geological age. While Prof. Sollas points out how the foliation in this case preceded the intrusion of the granite, we may take it that the same earth-movements produced both the one and the other at no long interval, the foliation being a prelude to the final yielding of the uptilted rocks.

The phenomena of foliation in the schists, and of flow-structure in the granite, may be studied by any visitor to Dublin upon the airy summits above Killiney Bay; but in some spots the granite itself has assumed a gneissic character through pressure subsequent to consolidation. On this point pp. 496–502 of the present memoir may be commended to workers in metamorphic areas; especially noteworthy is the insistance that the formation of cracks at right angles to the direction of flow, so often seen throughout the elongated constituents of a rock deformed by pressure, will serve under the microscope to distinguish cases of secondary from those of primary and igneous flow.

Among the interesting series of conclusions, we may remark how the soda-lime felspars predominate over the potash varieties in the Leinster granite. This only confirms what microscopic examination has taught us of a host of "granites"; so that we may have to fall back upon the comfortable definition of the rock as consisting of "quartz, felspar, and mica," unless we are willing to hand over many old acquaintances to the increasing group of the quartz-diorites.

Although the author states that we have no trace of volcanic ejectamenta emanating from the Leinster granite, we should be inclined to connect with it the great series of Ordovician "felsites," and associated tuffs stretching along its flanks from Wicklow southwards. Volcanoes characteristically break out upon the margins of an uplifted area, not upon its crest, where the eruptive energy may be presumed to become less and less; and highly silicated lavas, ancient obsidians and pumiceous tuffs, abound among the Ordovician rocks of Leinster, probably as the direct precursors of the granite.

Contributions such as these furnished by Prof. Sollas will be estimated at their full merit by those familiar with the long processes of isolation and analysis. The work of careful weeks may occasionally come before us in a page; and the results may appeal more immediately to the philosophic chemist and the crystallographer. But from such a memoir, as from that recently presented to the Geological Society by Messrs. Marr and Harker, we may learn what work lies before us even on familiar granite fells; and we may turn with renewed zest from the streets of Dublin to its highlands, to the moorland white with hoar-frost, and the broad glens reaching to the sea.

G. C.

THE FLORA OF WARWICKSHIRE.

The Flora of Warwickshire. "The Flowering Plants, Ferns, Mosses, and Lichens," by James E. Bagnall, Associate of the Linnean Society." "The Fungi," by W. B. Grove, M.A., and J. E. Bagnall. 518 + 34 pages, with a Map. (London: Gurney and Jackson. Birmingham: Cornish Brothers, 1890.)

THE interest to outsiders in the plants of Warwickshire lies in the fact that we have here a typical English Midland county, the botany of which is not in any way affected by proximity to the sea or high mountains. Although it is the central county of England, and forms the watershed of the Severn, Trent, and Thames, no portion of its surface rises above 855 feet. Its area is 885 square miles, or 566,458 acres, and it contains 4 hundreds, 2 cities, 1 county town, 10 market towns, and 209 parishes. In 1888 the crops of corn, beans, and peas occupied 106,000 acres; green crops, 38,602 acres; permanent pasture land, 312,000 acres; fallow, 8161 acres; and woodland, 16,650 acres. The soils are fertile, but varied, comprising nearly all kinds but those containing chalk and flints. All the southern and south-eastern part of the county is occupied by a strong clay resting on limestone. A soil of similar kind occupies the north-west. Over a large portion of the county, extending from Warwick to its western boundary, are strong clay loams resting on marl and limestone. Westward and south-westward of Warwick there is a strong clay over limestone. About Rugby and in the valleys of the Blythe and Tame are light sandy soils mixed with gravel. The remaining extensive portions of the county consist of a red sandy loam and a red clay loam, resting on freestone or limestone, and sometimes on gravel. The extent of uninclosed land is very inconsiderable, the only extensive commons being those of Sutton Coldfield and Yaningale. The subjacent sedimentary rocks begin with the Cambrian and end with the Inferior Oolite, with a little volcanic tuff with intrusions of diabase and quartz porphyry in the north-east, near Atherstone.

The author of this book, Mr. James Bagnall, is one of the most meritorious and best-known of our working-men naturalists. He lives in Birmingham, and has devoted himself specially for the last twenty years to the study of the botany of his native county, and in the present work the result of his long and diligent labours is carefully summarized. He has taken rank as one of our best critical British botanists, and has been selected by the Linnean Society as one of its fifty Associates, and has been awarded the Darwin Medal given by the Midland Union of Natural History Societies for the encouragement of original research. His fellow-townsmen are justly proud of him, and when the present work was planned, a number of local gentlemen, with Mr. Joseph Chamberlain at their head, undertook to guarantee him against pecuniary loss. There was, however, no need to call upon them, as 430 out of the 500 copies printed were subscribed for before the book was issued.

The work consists of an introduction of thirty-one pages, which contains the needful explanations of the plan followed in the enumeration of plants and their distribution, together with a sketch of the physical geography, meteorology, and geology of the county, the latter

contributed by Mr. A. Bernard Badger. The great body of the work—328 pages—is taken up by the enumeration of the flowering plants and vascular Cryptogamia that grow in the county, and an account of their distribution and the special stations of the rarities. The county is divided into ten districts founded on the river drainage, and each of these has been worked separately. The last edition of the "London Catalogue" has been followed as a standard of nomenclature and species limitation. The county is specially rich in Rubi, and in studying them Mr. Bagnall had the advantage in starting of the oversight in the field of the Rev. A. Bloxam, who was one of the best practical authorities in this difficult genus that we have had in this country. The flowering plants of the county have been worked so thoroughly that it is not likely that any material additions will be made. Then follows the enumeration of the mosses, of which 236 species are known in the county. The Hepaticæ and lichens have not been worked so carefully, and in these orders there is ample scope for further research. The enumeration of the fungi is confined to the Hymenomycetes and Gasteromycetes. The enumeration of the lower Cryptogamia occupies 130 pages. Then follows a table showing the distribution of the plants through the ten drainage districts of the neighbouring counties of Leicester, Northampton, and Oxford. The book concludes with a sketch of the progress of botanical investigation in the county; the principal botanists who have worked within its limits being Withering, Stokes, Perry, Purton, Bree, and Bloxam.

The flowering plants and vascular Cryptogamia of the county summarize as follows:—Out of 532 plants generally diffused throughout Britain, Warwickshire has 501. Out of 409 species concentrated towards the south of Britain, Warwickshire has 285. Out of 127 plants concentrated in the eastern counties, Warwickshire has 31. Out of 70 plants concentrated in the western counties, Warwickshire has only 8. Out of 37 plants concentrated in the centre of Britain, Warwickshire gets 7. Out of 208 plants which represent the boreal element in the British flora, Warwickshire has only 19.

The book is not too large to be conveniently carried, which is a great advantage in a county flora. From every point of view it is thoroughly satisfactory, and will be a lasting memorial of the ability and diligence of its author.

J. G. BAKER.

OUR BOOK SHELF.

A Hand-book and Atlas of Astronomy. By W. Peck, F.R.S.E., F.R.A.S. (London and Edinburgh: Gall and Inglis, 1890.)

AS Astronomer and Public Lecturer to the City of Edinburgh, the author of this work might reasonably be expected to be familiar with the requirements of a popular hand-book of astronomy. His aim, however, has not been to give a mere outline of the subject, but to give "complete and accurate" information in the principal departments of modern astronomy. In this endeavour he has compiled the volume before us, consisting of 170 pages, and embellished with 20 large plates and numerous smaller diagrams. For the ordinary reader who does not possess even a small telescope, the book has not much to recommend it. The descriptions are often very meagre, and the spectroscopic work which is now engrossing the attention of so many astronomers is scarcely touched upon. The star maps and the tables which accompany them are excellent, but it is questionable whether they would not have been more convenient if issued separately, instead of forming part of a rather bulky volume. Yet, if these were taken away, there could be little excuse for the existence of the remainder. That is to say, there would be little left that is not already available in much cheaper forms. The author has fallen into the common error of attempting to combine a popular work, suited to the general reader, with one more especially adapted for those wishing to acquire a comprehensive knowledge of the subject. From either point of view, the deficiency of spectroscopic astronomy is very conspicuous.

Some of the large diagrams are really excellent, but others are very indifferent. Amongst the latter the most striking are Plate 9, illustrating solar phenomena, and Plate 19, depicting various spectra. In the latter the colours are unsatisfactory, and the spectrum of hydrogen is represented as consisting of two bright lines and numerous dark ones. It would have been a great improvement if, instead of the drawings of some of the brightest nebulæ, photographs had been reproduced. The reproductions of photographs of the moon, taken by the author, are excellent.

Biographisch-litterarisches Handwörterbuch der wissenschaftlich bedeutenden Chemiker. By Carl Schaedler. (Berlin: R. Friedlaender und Sohn, 1891.)

BIOGRAPHICAL notes of some hundreds of chemists and physicists are here collected together, the names being arranged in alphabetical order. In the majority of cases there are given the date and place of birth, and, in cases where it has occurred, of death, besides the offices held, the most important of the work done, and the books, &c., written by the individual. The period covered extends from before the Christian era, and among the most recent names may be found those of Thomas Carnelley and Sydney Gilchrist Thomas. The collection cannot fail to be useful and interesting, but its value to historians would have been greatly enhanced if references had been given to the authorities from which the statements are derived. To do this would have probably added but little to the trouble of compilation, and would have made the volume a standard work of reference.

Round Games with Cards. By Baxter-Wray. (London: George Bell and Sons, 1891.)

IN this little treatise, Mr. Baxter-Wray deals with all the most popular round games with cards. Among them may be mentioned nap, loo, poker, vingt-un, commerce, pope-joan, spin, together with eight others which are played at the present day. With regard to each game the reader receives sound advice as to the methods he should adopt, and those he should not. The variations of each game are well described, but mention might have been made—in the variation of nap called "misery, or misère"—of playing with all the hands on the table face upwards, which affords, when more than three are playing, an excellent game requiring much skill and tact.

Many suggestions and rules are given pertaining to the stakes, deals, &c.; and those who read the book will find in it all that will enable them to learn a new game.

Elementary Science Lessons: Standard II. By W. Hewitt. (London: Longmans, Green, and Co., 1891.)

THIS book is intended to be in the hands of teachers, who, by making a judicious use thereof, should be able to engraft much in the minds of young people in a sound and practical manner. The principle on which it is written is excellent. The work is drawn up on the same lines

as the first series, only it is of a slightly more advanced character. The idea throughout is to place objects before the children, by means of which they may be able to recognize the general properties relating to them. Thus, in the first few lessons certain substances are exhibited, from which the general idea of solids, liquids, and gases can be gathered. The general characters of iron and steel, and those of a variety of other metals, are then illustrated, the metallic surfaces of which suggest the principles of the reflection of light, which are consequently treated of. The remaining lessons deal with sunlight, colour, motion, and the forces that produce it. The appliances for the experiments are of the most simple kind, and there are notes for the use of the teacher, from which the necessary information can be gathered.

LETTERS TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. *No notice is taken of anonymous communications.]*

Darwin on the Unity of the Human Race.

HAVING had occasion last year to allude as a fact to the circumstance that Charles Darwin assumed mankind to have arisen at one place, and therefore in a single pair, I was surprised to find that this fact was doubted, or at least very doubtfully accepted, by some of my scientific friends; and I was asked for a reference to his works in confirmation of it. My principal reliance, however, was in the recollection of a private letter to myself from the illustrious naturalist, which I had unfortunately mislaid. Having now recovered this letter, I send a copy of it to NATURE for publication, simply explaining that this letter was in reply to a letter from me in which I put the direct question, why it was that he did assume the unity of mankind as descended from a single pair? It will be observed that in his reply he does not repudiate this interpretation of his theory, but simply proceeds to explain and to defend the doctrine.

ARGYLL.

"*Down, Beckenham, September* 23, 1878.

"DEAR DUKE OF ARGYLL,—The problem which you state so clearly is a very interesting one, on which I have often speculated. As far as I can judge, the improbability is extreme that the same *well-characterized* species should be produced in two distinct countries, or at two distinct times. It is certain that the same variation may arise in two distinct places, as with albinism or with the nectarine on peach-trees. But the evidence seems to me overwhelming that a well-marked species is the product, not of a single or of a few variations, but of a long series of modifications, each modification resulting chiefly from adaptation to infinitely complex conditions (including the inhabitants of the same country) with more or less inheritance of all the preceding modifications. Moreover, as variability depends more on the nature of the organism than on that of the environment, the variations will tend to differ at each successive stage of descent. Now it seems to me improbable in the highest degree that a species should ever have been exposed in two places to infinitely complex relations of exactly the same nature during a long series of modifications. An illustration will perhaps make what I have said clearer, though it applies only to the less important factors of inheritance and variability, and not to adaptation—viz. the improbability of two men being born in two countries identical in body and mind. If, however, it be assumed that a species at each successive stage of its modification was surrounded in two distinct countries or times by exactly the same assemblage of plants and animals, and by the same physical conditions, then I can see no theoretical difficulty to such a species giving birth to the new form in the two countries. If you will look to the sixth edition of my 'Origin,' at p. 100, you will find a somewhat analogous discussion perhaps more intelligible than this letter. "Yours faithfully,

"CHARLES DARWIN."

Prof. Van der Waals on the Continuity of the Liquid and Gaseous States.

THERE are many, no doubt, who will be pleased to have the English translation of some of the papers of Prof. Van der Waals which has recently been published by the Physical Society. There are those at any rate who will be glad to satisfy themselves, without overmuch labour, as to how much there is of real importance in these much-discussed memoirs, published originally in a language too little studied in this country. I do not propose to criticize the papers, though I do not think that either the thermodynamics or the conclusions will bear examination; but I cannot avoid the task, however ungracious, of pointing out that they do not show a proper appreciation of the work of Andrews.

I will make but two or three quotations, and they shall be as brief as possible.

On the first page of the author's preface appears this sentence: "These latter [theoretical considerations] led me to establish the connection between the gaseous and liquid condition, the existence of which, as I afterwards learned, had already been suspected by others." The author's preface concludes as follows: "That my conception has shown itself to be a fruitful one cannot be denied, and it may be the incentive to further inquiry and experimental investigation."

The claim put forward in these sentences appears to me absolutely untenable. This connection, or relation as it might better be called, was not only "suspected" by Andrews, but was clearly and explicitly stated by him in the Bakerian Lecture for 1869; a paper published under the very title which Van der Waals, in 1873, has taken (without a word of acknowledgment) as the title of his essay.

On p. 430 a description is given of the mode of altering the gaseous condition of a substance (carbonic acid) into the liquid condition, and *vice versâ*, by a continuous process devoid of any abrupt change. At the end of the description come the two sentences: "Now, we cannot but call this substance a gas, though formerly we called it a liquid. I have borrowed this remark from Maxwell." The whole description was given by Andrews in the Bakerian Lecture (read June 17, 1869); and was referred to and accentuated by its author in the Royal Society Proceedings abstract of his complete paper.

The curves, Plate v., Fig. 3, are taken, says Prof. Van der Waals (p. 416), from Maxwell (Maxwell's "Theory of Heat" I understand from a reference a few lines higher on the same p. 416). This is to me unintelligible. The curves seem certainly not taken from Maxwell, but are somehow obtained from the original curves of Andrews (after a transformation, which Maxwell also makes, of turning Andrews's curves right for left); and they contain the peculiarity (purposely omitted by Maxwell, for simplicity) of a bend instead of a sharp corner at the bottom of the low temperature curves. In any case Maxwell credits Andrews with the construction of these remarkable curves, which contain, indeed, the germs of the whole *discovery of continuity* made by Andrews and James Thomson. As to the curves themselves, it is utterly unintelligible that anyone with a true perception of their physical meaning should allow the isothermal marked 25°·5 to stand as part of the diagram. The translators ought to have corrected or cancelled this on the ground which led them to object, in the footnote, p. 416, to an ill-judged remark in the text.

Throughout this essay on a subject which, by patient labour and consummate experimental skill, crowned with a rich harvest of results, Andrews made incontestably his own, there is not a single reference to the title or date, or existence even, of the Bakerian Lecture; nor, with the solitary exception of a very questionable reference on p. 421, is there a hint given that Andrews ever gave any attention to the question of continuity; and no uninformed reader would guess from this essay that Andrews had done anything more than supply a quantity of numbers which afterwards turned out to be convenient for the purpose of affording such confirmation as numbers can to the "discoveries" and "laws" of Prof. Van der Waals.

Whatever weight may be given to Van der Waals' investigation, no one who knows the subject as it was known in 1869 can fail to see that neither the idea nor the proof of continuity is in any sense whatever due to him. In their ultimate form they are due to Andrews and James Thomson; though of course it must never be forgotten that the whole subject was opened up by the investigations of Faraday and Cagniard de la Tour; and

that Donny, Dufour, and others made important contributions to the subject. I can find no vestige of a novel idea on the subject of continuity in the essay of Van der Waals. If he has succeeded in extending thermodynamic formulas to fit in with Andrews's discoveries, so far the work is praiseworthy and valuable; but an essay "On Continuity" ought certainly to contain a suitable acknowledgment to the author of the discovery.

J. T. BOTTOMLEY.

Rainbows on Scum.

I HAVE several times noticed "rainbows" on the black scum upon the pond in a park in this town, and have imagined all of them to have been formed on *dew* deposited upon a film of soot. It is out of the question, however, that yesterday there can have been any *dew* to produce the phenomenon, as there had been a thick hoar frost on the grass, all melted by the warm sun by 10.30 a.m., at which time there was a very vivid double "rainbow," which seemed exactly like an ordinary bow upon rain, except that there were none of the supernumerary arcs due to diffraction, and that the outer bow was fainter than usual in proportion to the inner one. The pond was thinly frozen over, but, it being a cloudless day, the surface of the ice was by that time covered with water. On closely examining the scum, I found it was composed of floating black particles, I presume of soot (the weather being rather foggy), and to many of these, minute drops were adhering, which varied much in size, the largest being probably $\frac{1}{80}$ of an inch in diameter. It was surprising to find distinct drops upon water, but I suppose it must have been the particles of soot that kept them separate. It seems probable they were a portion of the melted hoar frost; but it is rather curious that in such a situation this can produce a rainbow, seeing that usually melted hoar frost does not do so at all, or at most gives a very slight one; so decidedly is this the case, that one may distinguish in the morning between dew and melted hoar frost by noticing whether a "rainbow" and white anthelion are formed; dew being capable of producing bright ones owing to the roundness of the drops composing it, while hoar frost when it melts usually turns into irregular drops. I may say, however, that this "rainbow" on the pond was far more brilliant than any ordinary dew bow, and therefore it would appear that there is some property in the particles of soot to perfect the roundness of drops adhering to them, and so produce a striking phenomenon even from melted hoar frost.

At 0.30 p.m. I noticed the bow was fainter, but still fairly bright, and I estimated there were about 100 drops to each square inch of the surface of the water; it seems this number of drops, averaging probably not more than 0·003 to 0·005 inch in diameter, is sufficient to produce a pretty bright "rainbow." When I placed my eye close to individual drops I found that supernumerary arcs were visible.

T. W. BACKHOUSE.

Sunderland, February 20.

Wild Swine of Palawan and the Philippines.

DR. A. NEHRING has recently (*Abhandl. Mus. Dresden*, 1889, No. 2, p. 14 *et seq.*) characterized the wild pig of Luzon under the title of *Sus celebensis* var. *philippensis*, and the animal found in the island of Palawan (Paragua) as *Sus barbatus* var. *palavensis*. It would seem that both these local races or species have been already characterized and figured by M. Huet from specimens collected by M. Alfred Marche—the first-named as *Sus marchei* (*Le Naturaliste*, 1888, p. 6), and the second as *Sus ahenobarbus* (*op. cit.*, p. 5).

It is interesting to note that the wild pig of Palawan is a representative of the well-marked *Sus barbatus* of Borneo, and not of the wart-faced animal of the Philippines proper.

No specimens of wild pig from the large island of Mindanao appear to have been examined as yet. The following note of the external characteristics of a boar's head brought to me at Zamboanga may therefore be worth being transcribed:—"It was black with a white bar across the snout half-way between the eyes and the nose, with a black spot at the inner corner of the eyes; two tufts of coarse white bristles on the fleshy protuberances on the cheeks on either side, and two singular fleshy black knobs (warts) on the sides of the snout just below the white band."

A. H. EVERETT.

41 York Terrace, Regent's Park.

A Beautiful Meteor.

ON February 25, about 7.30 p.m., I chanced to see, at Coombe, near Woodstock, Oxfordshire, a globular meteor start from the Pointers, and fall with a slightly northerly inclination. When it was near the horizon, my garden wall hid from my sight the close of its brilliant career. Big as Venus at her brightest, it was substantially of a yellow colour, but shot over with flashes of glowing scarlet.

JOHN HOSKYNS-ABRAHALL.

Coombe, near Woodstock, Oxfordshire.

INFECTIOUS DISEASES, THEIR NATURE, CAUSE, AND MODE OF SPREAD.[1]

I.

WE read in Homer that "Phœbus Apollo, offended by mortals, sent a pernicious plague into the camp of the Greeks; the wrathful god with his arrows hit first mules, then dogs, and then also the Greeks themselves, and the funeral pyres burned without end." If we expressed this in less poetical language, but more in conformity with our modern realistic notions, we would say, that the deity of health and cleanliness, having been offended by mortals, sent his poisonous but imperceptible darts or bacilli into them, and caused an epidemic of a fatal disease, communicable to man and animals.

In whatever form we meet with this simile—whether an epidemic be ascribed to a wrathful providence, or to a sorcerer or a witch that put their spells on man or on cattle, thereby causing numbers of them to sicken and to die; whether this happened amongst the nations of old, or amongst the modern Zulus, whether amongst the peasants of Spain or in Italy—we now know that it always means that the offended deity of cleanliness, and the outraged laws of health, avenge themselves on mortals by the invasion of armies of imperceptible enemies, which we do not call arrows, nor sorcerers' or witches' spells or incantations, but microbes.

From Homer's Trojan epidemic among the Greeks to the epidemic in the camp of Cambyses, from the plagues carried and spread by the Crusaders of old to the plagues carried and spread in modern times by pilgrims to and from Mecca, the plagues following the ancient armies and those of more recent times, the plagues attacking a country debilitated by famine or by superstition have been in the past, and will be in the future, due in a great measure to neglect and ignorance, on the part either of individuals or of a whole population, of the principles of the laws of health: and it is chiefly to this neglect, ignorance, and indolence that the spread and visitations of epidemic infectious disorders must be ascribed. It is, therefore, with justice, that these disorders are called preventable diseases, and one cannot imagine a greater contrast than that between the knowledge we possess at the present time of communicable diseases, as to their cause, mode of spread and prevention, and the views of former generations, as to their spontaneous origin.

Although the notion that all epidemic diseases are communicable, *i.e.* spread from one individual to another, is not a new one, since many writers of former generations have had clear ideas about them, yet the actual demonstration of the fact that the different infectious or communicable diseases are due to definite species of microbes, which, having invaded a human or animal organism, are capable therein of multiplying and of causing a particular infectious illness; further, the identification of these living germs in the blood and tissues of an invaded individual, and the recognition of their many and intricate migrations outside the animal body; the study of the microbes in artificial cultures, *i.e.* out-

[1] Lecture delivered at the Royal Institution on Friday, February 20, 1891, by E. Klein, M.D., F.R.S., Lecturer on Physiology at St. Bartholomew's Hospital Medical School.

side the human or animal body; further, the best means to do battle with them, to neutralize them, to prevent their growth and to destroy them; then the *modus operandi* of the different species, each appertaining to, and causing, a definite kind of disorder—in short, all that is exact and precise in the knowledge of the causation, nature, and prevention of infectious diseases, is an outcome of investigations carried on during the last twenty-five years. Modern research has not only definitely demonstrated these microbes, it has also shown that a number of diseases not previously suspected as communicable have a similar cause to the above, and are therefore now classed amongst them. It need hardly be emphasized that a knowledge of the causes must lead, and as a matter of fact has led, to a clearer and better understanding of the recognition, prevention, and treatment of these disorders, an understanding obviously directed towards, and followed by, the alleviation and diminution of disease and death in man and animals. I may point to a few special examples to illustrate these propositions. The disease known as splenic apoplexy or malignant anthrax is a disease affecting man and brutes. In some countries the losses to agriculturists and farmers owing to the fatal character of the disease in sheep and cattle is enormous. In man it is chiefly known amongst wool-sorters and those engaged in the handling of hides. This disease has been definitely proved to be due to a bacillus, the *Bacillus anthracis*, which, after its entry into the system of an animal or human being, multiplies very rapidly in the blood and spleen, and, as a rule, produces a fatal result, at any rate in sheep and cattle. Now, the bacillus having been proved to be always associated with this disease, anthrax, it was then shown that this bacillus can grow and multiply also outside the animal body: its characters in artificial media have been carefully studied and noted, so that it can be easily recognized; and by the pure cultures of the bacillus the disease can be again reproduced in a suitable animal. Such cultures have been subjected to a number of experiments with heat, chemicals, or antiseptics; the chemical function of the *Bacillus anthracis* has been and is being accurately studied in order to give us an insight into the mode in which it is capable of producing the disease; it has been further shown by Koch that the bacilli are capable of forming seeds or spores which possess a very high degree of resistance to various inimical conditions, such as heat, cold, chemicals, &c., and that it is precisely these spores, entering the system by the alimentary canal, through food or water, or by the respiratory organs through the air, to which the disease in most instances is due. Further, it has been shown that a trace of the blood of an animal affected with or dead from the disease, when introduced into an abrasion of the skin of man or animal, produces at first a local effect (carbuncle) followed by a general and often fatal infection. But the most important result of the cultivation of the bacillus outside the body, in artificial media, was the discovery that if subjected to or grown at abnormally high temperatures, 42°·5 C., *i.e.* above the temperature of the animal body, its power to produce fatal disease—that is, its virulence—becomes attenuated, so much so that, while the so-altered bacilli on inoculation into sheep or cattle produce a mild and transitory illness, they nevertheless furnish these animals with immunity against a fatal infection.

The recognition and identification of the *Bacillus anthracis* as the true cause of the disease, splenic fever or splenic apoplexy, the knowledge of its characters in the blood and spleen of man and animals, and of its peculiarities in artificial cultures, have enabled us to make a precise diagnosis of the disease, which previously was not always easy or even possible. The knowledge of its forming spores when grown under certain conditions, and of the manner in which experimentally the disease can be reproduced in animals by the bacilli and its spores, has led to a complete understanding of the means and ways in which the disease spreads both in animals and from them on to man; and last, but not least, the methods of the protective inoculations first indicated and practised by Pasteur have been solely the result of the studies in the laboratory of the cultures of the *Bacillus anthracis*, and of experiments with them on living animals. I could add here a number of other diseases, such as glanders, fowl cholera and fowl enteritis, erysipelas, scarlet fever and diphtheria in man, actinomycosis in man and cattle, swine fever and swine erysipelas, grouse disease, symptomatic charbon in cattle, and other diseases of animals—we have been brought to a fairly advanced understanding of one and all of these by methods such as those indicated above; and hereby not only in the diagnosis and recognition, but also in the treatment and prevention of these disorders, an immense amount of valuable progress has been achieved.

[1. Demonstration: lantern slides of anthrax, fowl cholera, fowl enteritis, grouse disease, typhoid, cholera, pneumonia, diphtheria, actinomycosis, scarlatina, and glanders.]

As examples of the second proposition, viz. that the modern methods of study of disease germs, of their nature and action on living animals have led to the recognition as communicable diseases of some disorders which previously were not known or even suspected to be of this character, I may mention amongst several the disease known as tuberculosis or consumption, tetanus or lock-jaw, and acute pneumonia. Not until Klencke and Villemin had shown by direct experiment on animals that tuberculosis is inoculable was it grouped amongst the infectious diseases. Since these experiments were first published a large amount of work has been done, proving conclusively that tuberculous material—that is, portions of the organs containing the tubercular deposits (*e.g.* lung, lymph gland, spleen, &c.)—by inoculation, by feeding or by introducing it into the respiratory tract, can set up typical tuberculosis in the experimental animals; the tubercular deposits in these experimental animals again are endowed with the power to propagate the disease in other animals. Further, it was shown that the disease in cattle called "Perlsucht" was in all respects comparable to tuberculosis in man, and it is accordingly now always called tuberculosis.

A further, and perhaps the greatest, step was then made by Koch's discovery in 1882 of the tubercle bacillus, and his furnishing the absolute proof of its being the true cause of the disease. The demonstration and identification of this microbe is now practised, I might almost say, by every tyro, and it is of immense help to diagnosis. In former years, and before 1882, the diagnosis of tuberculosis was not by any means an easy matter in many cases of chronic lung disease; since that year every physician in such cases examines the expectoration of the patient, and the demonstration of the tubercle bacilli makes the diagnosis of tuberculosis absolutely certain. Not only in medical, but also in many surgical cases, *e.g.* certain forms of chronic disease of bones and joints, particularly in children, the demonstration of the tubercle bacilli is of essential importance, and by these means diseases like lupus of the skin, scrofula and certain diseases of bones and joints not previously known as tuberculosis, are now proved to be so. The same applies to animals; wherever in a diseased organ of man or animal the tubercle bacilli can be demonstrated, the disease must be pronounced as tuberculosis.

The proof that the tubercle bacillus is the actual cause of the tubercular disease was established by Koch beyond possibility of doubt. Cultures in artificial media were made from a particle of a tubercular tissue either of a human being, or of cattle affected with tuberculosis, or of an experimental animal tubercular by ingestion, or by

injection with tuberculous matter, and in all cases crops of the tubercle bacilli were obtained. Such cultures were then carried on from subculture to subculture, through many generations, outside the animal body; with a mere trace of any of these subcultures, however far removed from the original source, susceptible animals were infected, and all without fail developed tuberculosis, with the tubercle bacilli in the morbid deposits of their organs. The discovery of the tubercle bacilli and the demonstration that they are constantly present in the tubercular deposits of the typical tuberculosis, and the proof by experiment on living animals that they are the actual cause of the disease, are not all that we have learned, for it has also been shown that certain diseases, like the dreaded and disfiguring disease known as lupus—at any rate some forms of it—and further the disease scrofula, so often present in children, are really of the nature of tuberculosis, the former in the skin, the latter in the lymph glands.

Now see what an enormous step in advance this constitutes:--

(1) We can now diagnose tuberculosis with much greater accuracy in man and animals, even in cases in which this was formerly difficult or impossible.

(2) We have accepted rightly that all forms of tuberculosis are infectious or communicable diseases, communicable by inoculation, by ingestion, *i.e.* by food, or by respiration, *i.e.* by air.

(3) We have learned to recognize that, as in other infectious disorders, there exists a risk to those susceptible to tuberculosis, of contracting the disease from a tubercular source, and it is the recognition of these facts which ought to regulate all efforts to prevent its spread.

Tetanus or lockjaw, not previously known to be so, has likewise been fully demonstrated to be an infectious disease: we now know that it is due to a bacillus having its natural habitat in certain garden earth; that this bacillus forms spores, that these spores gaining access to an abrasion or wound of the skin in man or animals are capable of germinating there and multiplying, and of producing a chemical poison which is absorbed into the system, and sets up the acute complex nervous disorder called lockjaw. The recognition of the disease as an infectious disease and caused by a specific microbe has taught us at the same time the manner in which the disease is contracted, and thereby the way in which the disease is preventable.

[2. Demonstration: lantern slides of tubercle and tetanus.]

The study of disease germs by the new and accurate methods of bacteriology has also led to a clearer and better understanding of the manner in which at any rate some of the infectious diseases spread. While it was understood previous to the identification of their precise cause that some spread directly from individual to individual (*e.g.* small-pox, scarlet fever, diphtheria), others were known to be capable of being conveyed from one individual to another indirectly, *i.e.* through adhering to dust, or being conveyed by water, milk, or by foodstuffs (*e.g.* cholera, typhoid fever). But we are now in a position to define and demonstrate more accurately the mode in which infection can and does take place in many of the infectious diseases. By these means we have learned to recognize that the popular distinction between strictly contagious and strictly infectious diseases—the former comprising those diseases which spread as it were only by contact with a diseased individual, while in the latter diseases no direct contact is required in order to produce infection, the disease being conveyed to distant points by the instrumentality of air, water, or food—is only to a very small extent correct. Take, for instance, a disease like diphtheria, which was formerly considered a good example of a strictly contagious disorder; we know now that diphtheria, like typhoid fever or scarlet fever, can be, and, as a matter of fact, is, often conveyed from an infected source to great distances by the instrumentality of milk. In malignant anthrax, another disease in which the contagium is conveyable by direct contact, *e.g.* in the case of an abrasion or wound on the skin coming in contact with the blood of an animal dead of anthrax, we know that the spores of the anthrax bacilli can be, and, as a matter of fact, in many instances, are, conveyed to an animal or a human being by the air, water, or food. The bacilli of tubercle, finding entrance through a superficial wound in the skin or mucous membrane, or through ingestion of food, or through the air, can in a susceptible human being or an animal produce tuberculosis either locally or generally. The difference as regards mode of spread between different diseases resolves itself merely into the question, Which is, under natural conditions, the most common mode of entry of the disease germ into the new host? In one set of cases, *e.g.* typhoid fever, cholera, the portal by which the disease germ generally enters is the alimentary canal; in another set an abrasion or wound of the skin is the portal, as in hydrophobia, tetanus, and septicæmia; in another set the respiratory organs, or perhaps the alimentary canal, or both, are the paths of entrance of the disease germ, as in small-pox, relapsing fever, malarial fever; and in a still further set the portal is just as often the respiratory tract as the alimentary canal, or a wound of the skin, as in anthrax, tuberculosis. But this does not mean that the virus is necessarily limited to one particular portal, or that it must be directly conveyed from its source to the individual that it is to invade. All this depends on the fact whether or not the microbe has the power to retain its vitality and virulence outside the animal or human body.

Anthrax bacilli are killed by drying: they gradually die off if they do not find sufficient nutriment in the medium into which they happen to be transferred; they are killed by exposure to heat far below boiling-point; they are killed by weak carbolic acid. But if these anthrax bacilli have been able to form spores, these latter retain their vitality and virulence when dried, when no nutriment is offered to them, and even when they are exposed for a few seconds to the heat of boiling water, or when they are exposed to the action of strong solutions of carbolic acid. Similarly, the bacillus of diphtheria when dried is killed also by weak solutions of carbolic acid; it is killed when kept for a few days in pure water on account of not finding sufficient nutriment; fortunately the diphtheria bacillus is killed in a few minutes at temperatures above 60° or 65° C., for this bacillus does not form spores. The same is the case with the microbe of scarlet fever.

The tubercle bacillus forms spores; these are not killed by drying, they are killed by the heat of boiling water of sufficiently long duration, two or more minutes; they are not killed by strong carbolic acid.

While, therefore, we know in these cases on what the conditions of infection depend, we have also learned to understand the conditions which favour or prevent the infection.

Not all infectious diseases which have been studied are due to Bacteria: in some the microbe has not been discovered, *e.g.* hydrophobia, small-pox, yellow fever, typhus fever, measles, whooping-cough; in others it has been shown that the disease is due to a microbe which belongs, not to the Bacteria, but to the group of those simplest animal organisms known as Protozoa. Dysentery and tropical abscess of the liver are due to Amœbæ; intermittent fever or ague is due to a protozoon called Hæmoplasmodium; a chronic infectious disease prevalent amongst rodents, and characterized by deposits in the intestine, liver, and muscular tissue, is due to certain forms known as Coccidia, or Psorospermia. A chronic infectious disease in cattle and man known as actinomycosis is due to a fungus, the morphology of which indicates that it probably belongs to the higher fungi; certain species of moulds (*e.g.* certain species of Aspergillus and Mucor) are also known to be capable of producing definite

infectious chronic disorders, and so also is thrush of the tongue of infants; ringworm and certain other diseases of the hair and skin are known to be due to microbes allied to the higher fungi.

The microbes causing disease which have been studied best, are those belonging to the groups of Bacteria or Schizomycetes or fission fungi (they multiply by simple division or fission); most species of these have been cultivated in pure cultures, and the new crops have been utilized for further experiments on animals under conditions variable at the will of the experimenter.

[3. Demonstration: cultures of Bacteria in plates and in tubes.]

(*To be continued.*)

RECENT PHOTOGRAPHS OF THE ANNULAR NEBULA IN LYRA.

ANNULAR nebulæ can no longer be regarded as a class completely apart. They should rather be described as planetaries in which one special feature predominates over the rest. The progressive improvement of telescopes has tended to assimilate the two varieties by bringing into view peculiarities common to both. It is only when they are ill seen that planetary nebulæ appear really such. The uniformity of aspect at first supposed to characterize them disappears before the searching scrutiny of a powerful and perfect instrument. The usually oval surfaces which they present are then perceived to be full of suggestive detail. They are broken up by irregular condensations, or furrowed by the operation of antagonistic forces; betray here the action of repulsive, there of attractive, influences; and bear as yet undeciphered inscriptions of prophetic no less than of commemorative import. Among the various modes of diversification visible in them, however, two are especially conspicuous—first, the presence of a nucleus; next, the emergence of a ring, or even of a system of rings.

Now the nebula in Lyra, when distinguished by Sir William Herschel in the annular form, of which it is the most perfect exemplar, seemed completely perforated by a dark opening; but interior nebulosity, constituting the object essentially a disk with annular condensation, was noticed by Schröter in 1797, and is depicted as strongly luminous in the drawings of Lassell (reproduced in *Knowledge*, November 1890), Trouvelot (*Harvard Annals*, vol. viii., Plate 34, 1876), and Holden.[1] Moreover, a minute central star, visually discerned at intervals, has lately been photographed in unmistakably nuclear shape. The entire formation, then, consists of a disk, nucleus, and ring, and differs from many planetaries only in the proportionate lustre of its parts.

The records concerning the central star include some curious anomalies. They go back to the year 1800, when Von Hahn was struck with its disappearance. The change was in his opinion due, not to loss of light in the star, but to the veiling with delicate nebulous clouds of the dark background upon which, in former years, it had been seen projected (*Astr. Jahrbuch*, 1803, p. 106). His observations were made at Remplin, in Mecklenburg, with a 12-inch Herschelian speculum, somewhat impaired in brilliancy (Lisch, "Gesch. des Geschlechts Hahn," Bd. iv. p. 282). "Hahn's star" was next seen by Lord Rosse 1848 to 1851 (Trans. R. Dublin Society, vol. ii. p. 152), and drew the notice of Father Secchi in 1855 (*Astr. Nach.*, No. 1018). Twice observed by M. Hermann Schultz at Upsala in 1865 and 1867 ("Observations of 500 Nebulæ," p. 99), it unaccountably, ten years later, evaded the deliberate scrutiny with the Washington 26-inch of Prof. Asaph Hall, who nevertheless perceived the nebula as exteriorly surrounded by a "ring" of nine faint stars (*Astr. Nach.*, No. 2186). The same great instrument, however, displayed the missing star to Mr. A. C. Ranyard, August 23, 1878 (*Astr. Journal*, No. 200), while to Dr. Vogel, equally in 1875 with the Newall refractor, and on several nights in 1883 with the Vienna 27-inch, it remained imperceptible (*Potsdam Publicationen*, No. 14, p. 35). Very remarkable, too, is its non-appearance to Dr. Spitaler at Vienna in 1885, when he carefully delineated the nebula, as well as in 1886, during repeated verifying observations. The interior seemed then to contain only dimly luminous floccules; yet the star thus persistently invisible caught his eye at the first glance, July 25, 1887 (*Astr. Nach.*, No. 2800). It had in the meantime, September 1, 1886, been photographed by Von Gothard; and having committed itself to this *adsum qui feci* avowal, has, now for four years past, abstained from capricious disappearances.

Its variability, then, is still unproved; since observational anomalies, even of a very striking kind, may be explained in more ways than one; and it is very easy *not* to see a fifteenth-magnitude star. More especially when the sky behind it is—perhaps intermittently—nebulous. Luminous fluctuations in the diffused contents of the ring certainly suggest themselves to the student of its history. At times a bare trace of nebulosity is recorded; at others, the whole interior is represented as filled to the brim with light—mist, coagulated, as it were, into a cirrous or striated arrangement. Under such conditions, the effacement of quasi-stellar rays, feebly seen at the best, is not surprising.

The "gauzy" stuff within the ring possesses very slight actinic power. The best drawings of the nebula represent what might be described as an oval disk with a brighter border, while in all photographs hitherto taken it comes out strongly annular. The interior does not fill up even with such abnormally long exposures as might be expected to abolish gradations by giving faint beams time to *overtake* the chemical effect more promptly produced by brighter rays.

The photographic record of the Lyra nebula goes back a very few years. It opens in 1886, with some Paris impressions showing a small, nearly circular ring, starless, and perfectly black within. A decided advance was marked by Von Gothard's picture of a pair of nebular parentheses—thus, ( )—inclosing a very definite, though probably non-stellar, nucleus. The failure of light at each extremity of the major axis, which makes one of the most significant features of this object, was already in 1785 noticed by the elder Herschel (*Astr. Jahrbuch*, 1788, p. 242; Phil. Trans., 1785, p. 263); and Lord Rosse in 1863, and Schultz in 1865, were struck with its accompaniment, *on the north-eastern side*, by "nebulous radiations in the direction of the longer axis, which seemed momentarily almost to destroy the annular form." This appearance of an equatorial outflow, however, had shifted to the opposite or *south-westerly side* of the nebula when Holden observed it at Washington in 1875;[1] while a photograph taken by Mr. Roberts, with twenty minutes' exposure in July 1887, showed a very decided protrusion in the place indicated by Schultz, but only an abortive attempt at Holden's appendage. The suggestion of real changes of an alternating character, affecting luminosity perhaps, rather than figure, meets some confirmation in Prof. Holden's remark upon a further incompatibility between his own and Lord Rosse's observations on a different part of the same object. "It is a little curious," he wrote in 1876, "that that end of the minor axis which Lord Rosse has represented as the best terminated, viz. the *south*, is precisely that one which to-day, and with the Washington telescope, is least so" (*Monthly Notices*, vol. xxxvi. p. 63). And the Lick 36-inch similarly disclosed the whole of the bright southern edge as filamentous (*Monthly Notices*, vol. xlviii. p. 387) in opposite correspondence with the views obtained at Parsonstown of the northern edge.

[1] Executed in 1875, with a power of 400 on the 26-inch Washington refractor (*Wash. Observations* for 1874, Pl. vi.).

[1] *Monthly Notices*, vol. xxxvi. p. 66; and pastel drawing in *Wash. Obs.*, 1874.

The central star was recorded in fifteen minutes on Mr. Roberts's plates (*Monthly Notices*, p. 29); it waited 1h. 50m. to appear at Bordeaux. A picture, however, taken there in three hours by M. Courty in July 1890 included all Lord Rosse's seven exterior stars (Rayet, *Comptes rendus*, 7 Juillet, 1890); and Admiral Mouchez's suspicion that the nuclear one was inclosed in a quadrilateral of much fainter objects was verified, as regards three of the four, by a subsequent photograph obtained at Algiers by MM. Trépied and Rabourdin, with two exposures of three hours each, for which the international charting instrument of 13 inches was successfully employed.[1] In the resulting impression, Hahn's "star" is distended into a nucleus far more luminous than the fainter parts of the ring, and contrasting powerfully with a black intermediate space, usually grey and glimmering to telescopic vision. The ring itself comes out broad, hazy, and far from uniformly illuminated. Two somewhat unequal maxima and two conspicuous minima of brilliancy mark the extremities respectively of its minor and major axes. Its outline, though blurred, is tolerably symmetrical. No effusions interrupt, no "fringes" obliterate it.

The method of multiple exposures (introduced by Mr. Roberts) enabled, in November last, the Toulouse astronomers, MM. Andoyer and Montangerand, to get sittings from the Lyra nebula for a single portrait, summing up to nine hours, with the result of bringing out, over an area of three square degrees, about 4800 stars, including, of course, the nuclear *punctum saliens*. And the registration of this stellar multitude is the more valuable from the probability that some of them may belong to the same system with the strange object round which they swarm. Its inclosure by a "ring of stars" was remarked by Prof. Hall in 1887; Prof. Holden in 1888 perceived indications of a second similar but interior ring, evidently forming part of a complex nebular structure. The places of twelve of its minute components were measured by him; and it will be interesting to learn whether they can be identified in the Toulouse photograph.

An irresistible inference from the data collected, both visually and photographically, concerning the Lyra nebula, is that its ellipticity is genuine, and not merely an effect of foreshortening. For it would be absurd to suppose the plan of construction of a sidereal body related in any way to its situation as regards the line of sight from the earth; hence, geometrical shape that is emphasized by inequalities of light must correspond to a physical reality. It is certainly by no accident that the transverse axis of the object in question terminates in maxima, its longitudinal axis in minima of brightness; and this alone conveys a positive assurance that it *is*, and does not simply appear oval.

More especially when we find that this very peculiarity is shared by several other nebulæ of the same class (Secchi, "Les Étoiles," t. ii. p. 13), and forms a link of a profoundly suggestive kind between them all and the great Dumb-bell Nebula. For this remarkable object, too, shows minima of illumination evidently homologous with those of the pattern ring-nebula; they affect corresponding parts of the structure, and depend, there can be little doubt, in a similar manner, upon internal organization. They are besides in each case attended by a symptom which may prove instructive as to their mode of origin. Mr. Roberts's photographs of the Dumb-bell, no less than of the annular nebula, bring into view dim effusions of nebulosity in the directions of its greatest extent. The elliptical outline is transcended at the two opposite points where it is partially interrupted by gaps of comparative obscurity. Now, the late Mr. Roche, of Montpellier, demonstrated that the "limiting surface" of a cometary mass falling towards the sun must assume the shape of an oval pointed along the line of junction with the sun (*Mémoires de l'Acad. de Montpellier*, t. ii. p. 426). This surface, representing the boundary of the atmosphere controllable by the comet, necessarily contracts in proportion as the solar attraction gains predominance; while the matter thus progressively abandoned, instead of escaping indifferently in all directions, flows away solely along recurving lines from the nodal points (as they might be termed) of the cometary envelope. The question then presents itself whether both the oval figure, and the apparent effluences along the major axes of the nebulæ we have been considering, may not be due to attractive influences in their neighbourhood. A gradual abandonment of nebulous material, at some previous time fully incorporated with the central mass, seems at any rate indicated, and cannot otherwise be easily accounted for. A. M. Clerke.

[1] *L'Astronomie*, t. ix. p. 441; Ranyard, *Knowledge*, November 1890; *C. rendus*, t. cxi. No. 15.

ZITTEL'S "PALÆONTOLOGY"—REPTILES.[1]

In the annual address to the Geological Society delivered last year by the retiring President, the work of which a portion is now before us was referred to as "the superb palæontological compendium now being published by our distinguished Foreign Member, Prof. K. v. Zittel, a book that, I fear, no English publisher at present would feel justified in undertaking." It is with a feeling of admiration mingled with regret—of admiration for this splendid work, and of regret that its like cannot be produced in this country—that we quote these words; for it is unfortunately but too true that palæontological science, or, in other words, the zoology of past epochs of the earth's history, is not cultivated among us with anything like that zeal which its importance and interest merit. Indeed (although there are signs that this spirit is now passing away), we too often hear palæontology and palæontologists mentioned by those who ought to know better with a covert, if not with an open sneer. Palæontology, however, if studied in a proper philosophic spirit, must be the very groundwork of all our systems of zoological classification; and therefore all students of zoology—both recent and past—owe a deep debt of gratitude to the author of this "Palæontologie," which may be said to be the only work which is so comprehensive as to deal, not only with the general outlines, but also with the details of the science of which it treats.

The two parts forming the subject of the present notice deal with that division of Vertebrates for which Prof. Huxley proposed the name Sauropsida, or, in other words, Reptiles and Birds. No less than 427 pages of letterpress, illustrated by 298 woodcuts, are devoted to this portion of the work, which is alone sufficient to give some idea of the fulness with which the subject is treated. Since, however, there are some repetitions of the figures, the total number is somewhat less than that given above. A considerable proportion of these woodcuts are original, but others have been copied from the memoirs of Owen, Cope, Marsh, and others, as well as from the British Museum Catalogues.

In dealing with the difficult question of the arrangement of the orders of Reptiles, and the number of such orders which it is advisable to adopt, we think that in the former respect the author has not been altogether well advised. The orders adopted are nine in number, and are arranged in the following sequence, viz.: (1) Ichthyosauria; (2) Sauropterygia; (3) Testudinata; (4) Theromorpha; (5) Rhynchocephalia; (6) Lepidosauria; (7) Crocodilia; (8) Dinosauria; and (9) Pterosauria.

Now the first point that strikes us as incongruous in this arrangement is the position assigned to the Theromorpha (otherwise known as Theromora or Anomo-

[1] K. v. Zittel, "Handbuch der Palæontologie," Vol. III. Vertebrata, Parts 3 and 4 (Munich and Leipzig, 1889–90).

dontia). Thus if any one point more than another is firmly established in herpetology, it is the relationship of the Theromorphs to the Amphibians; and there can, accordingly, be no question but that the position of this order should be immediately following the last-named class. We venture, indeed, to think that the author has by no means sufficiently realized the peculiar structural features and the high morphological importance of the Theromorphs, which are so closely allied, on the one hand to Amphibians, and on the other to the primitive Mammalian stock. It must, however, be conceded in his favour that several memoirs on this group have appeared since this portion of the work before us was written; while the confusion that has existed up till a late period between the bones of the shoulder and pelvic girdles of these reptiles renders certain errors in regard to these points perfectly excusable. Still, the absence of any definite mention of a distinct third bone—the precoracoid—in the shoulder-girdle of many members of this group, which is of such extreme morphological importance, is an omission which cannot be lightly passed over. A fourfold division of the Theromorphs into sub-orders is adopted—namely, into Anomodonts, Placodonts, Pariasaurians, and Theriodonts. The Anomodonts are represented by the well-known African and Indian Dicynodonts, but the inclusion of *Phocosaurus* is a glaring error, since the pelvis so named should be referred to quite another group. Indeed, the author—probably misled by others—does not appear to be acquainted with the true construction of the shoulder and pelvic girdles of the Dicynodonts. For instance, the bone represented on p. 560, Fig. 506, *b*, as a Dicynodont coracoid (which could not, by the way, possibly fit on to the scapula in the same figure) is really a right ilium. Then, again, the pelvis and sacrum shown in Fig. 510 (p. 562) as belonging to *Dicynodon*, are referable to *Pariasaurus*. Again, the thrusting of the Placodonts between the Anomodonts and Pariasaurians is an arrangement that will certainly not commend itself to those who have attentively studied these groups; while the Pariasaurians, as the lowest and most generalized representatives of the whole order, should certainly have come before all the others. The absence of full information on the structure of this group at the date of publication must, however, here again defend the author. The Owenian division of the Theriodonts into single-nostriled and double-nostriled groups has not stood the test of fuller examination; and the mingling of the American Permian types with the typical African ones is certainly an ill-advised proceeding. A wise discretion is, however, exercised in not assigning any definite position to that most remarkable African genus described by Sir R. Owen as *Endothiodon*, the skull of which presents such a curious approximation to a Mammalian type in the structure of the palate. If we may venture to hazard a conjecture on this point, we think it probable that the mammal-like humerus described by another writer under the name of *Propappus*, together with a pelvis which has been provisionally associated with it, will both eventually prove to belong to *Endothiodon*; when this genus will take a place near *Pariasaurus* as one of the most mammal-like reptiles yet known to us.

In criticizing thus freely Prof. v. Zittel's treatment of the Theromorphs, we do so because this is obviously by far the weakest part of this section of the work, and one where the student ought to be advised of the necessity of seeking for other aids to his studies.

The pestilent doctrine that the Ichthyosaurs are primitive reptiles directly descended from fishes would seem to be the sole reason for placing them first of all the reptilian orders. We have, however, but little doubt that the position assigned them by Dr. Baur next the Rhynchocephalians best expresses their true relationship. In the classification of this order the species are arranged under the genera *Mixosaurus*, *Ichthyosaurus*, *Ophthalmosaurus*, and *Baptanodon*, according to the British Museum Catalogue of Fossil Reptiles, from which (as in the case of the next order) a large proportion of the illustrations have been borrowed.

In the Sauropterygia, as represented by the Nothosaurs and Plesiosaurs, the systematic arrangement is likewise practically the same as the one adopted in the work last mentioned, with the important exception that the small Triassic reptile known as *Mesosaurus* is transferred to the Rhynchocephalia, of which more anon. In connection with the illustrations of the osteology of this order, we may observe that the two figures of the shoulder-girdle given on p. 777, as well as the first of the two on the succeeding page, are not altogether correct; while Fig. 463, on p. 487, described as the skeleton of *Plesiosaurus dolichodirus*, really represents the type specimen of *P. hawkinsi*.

There is nothing especially to notice concerning the Nothosaurs. In massing under the name *Cimoliosaurus* all those post-Liassic Plesiosaurs in which the ribs of the neck unite with the bodies of the vertebræ only by a single articulation (Fig. 1), the author once more follows the plan proposed in the British Museum Catalogue. It might, however, have been stated somewhat more definitely that this is to some extent merely a provisional arrangement for the convenience of working, and that it may subsequently be found advisable to refer those forms in which the vertebræ have more or less concave terminal faces (like the one in the figure reproduced) to a different genus, or even to several genera. The separation of the Triassic genus *Pistosaurus* from the typical Nothosaurs to form a new family at the top of the order is an entirely new departure, the necessity for which we are not altogether prepared to admit.

We are not sure that in adopting the name Testudinata, in place of the more usual Chelonia, for the tortoises and turtles, the author is well advised, although we are not disposed to find fault for such a trifling matter. At the same time, we are glad to see the view entertained that the so-called Leathery turtles are really nothing more than an extreme development of the type to which the true turtles belong, and that they are, therefore, not entitled to form a distinct sub-order. The separation of the turtles of the London Clay as a family distinct from the *Chelonidæ* is, however, certainly not justifiable, since they come exceedingly close to the living Loggerhead; and even if such separation were justifiable, the name of the family would surely not be *Chelonemydidæ*. A more serious error is the inclusion of the generalized genus *Platychelys* in the *Chelydridæ*, with which it has nothing whatever to do, being allied to the well-known *Pleurosternum*, of the English Purbeck, which appears to represent the ancestral group from which both the modern Cryptodira and Pleurodira have taken rise. Prof. v. Zittel places this genus among the Pleurodira, where he makes no attempt to separate the various genera into families, although a workable classification has been already proposed elsewhere.

Omitting the Theromorphs, to which we have already referred, the next group in the series is that of the Rhynchocephalians, followed by the Lepidosauria (Lizards and Snakes). In the near association of these two orders we think the author is decidedly in the right, for it appears impossible to believe that Lizards are not the direct greatly modified descendants of the more primitive Rhynchocephalians. In the latter order the remarkable Permian genus *Palæohatteria*—the prototype of the New Zealand *Sphenodon*—is placed in the same family with *Proterosaurus*, which some authors even make the type of a distinct order. This family is followed by the *Mesosauridæ*, the members of which undoubtedly show signs of affinity with *Palæohatteria*, although we venture to think that their Sauropterygian relationships are

sufficiently manifest to override this. We cannot, moreover, refrain from surprise when we see that the author still retains the genus *Stereosternum* as distinct from *Mesosaurus*, when those that have examined specimens of each are puzzled to find even points of specific distinctness.

In including Lizards, Snakes, and Mosasaurs under one ordinal heading, the author follows the lead of some of those who have given especial attention to these groups. The substitution of the name Lepidosauria for the more generally used term Squamata is, perhaps, an improvement, since the latter has been also applied to the loricated Edentate Mammals. In placing, however, the Cretaceous *Dolichosaurus* with the true Lizards, we notice a marked error in judgment, since this genus clearly indicates the ancestral type of the Mosasaurs. We must equally deprecate the retention of the genus *Palæovaranus*, of the French Phosphorites, among the *Varanidæ*, since it has been fully proved that all the remains so named belong to *Anguidæ;* the *Varanidæ*, so far as we know, being unknown in the earlier Tertiaries of any part of the world.

The Crocodiles are divided into the three sub-orders of Parasuchia, Pseudosuchia, and Eusuchia, the recent proposal to separate the first (and probably the second) group as a distinct order not having been published at the time that the work went to press. The tempting view that the true Crocodiles (Eusuchia) can be divided into a Longirostrine and a Brevirostrine section—the one terminating in the Gharial and the other in the Alligator—is, we regret to see, adopted. The close structural resemblance in all essential details of these two reptiles is, however, on the face of it almost sufficient proof that they could not have originated from totally independent stocks. And the truth indeed appears to be that every group of Crocodiles at every epoch has developed both long- and short-jawed forms. Thus in the *Metriorhynchus* group, which is placed among the Longirostres, the genus *Suchodus* is as short-jawed as any Alligator; while *Geosaurus* itself can scarcely be called more long-jawed than many species of the genus *Crocodilus*. We fail also to see how the genera *Plesiosuchus* and *Dacosaurus* are to be distinguished from *Geosaurus*. Here may be mentioned incidentally a small error, very excusable in a foreigner, into which the author has fallen in regard to the magnificent series of reptilian remains from the Oxford Clay collected by Mr. A. N. Leeds, of Eyebury, near Peterborough. Confusing this gentleman's

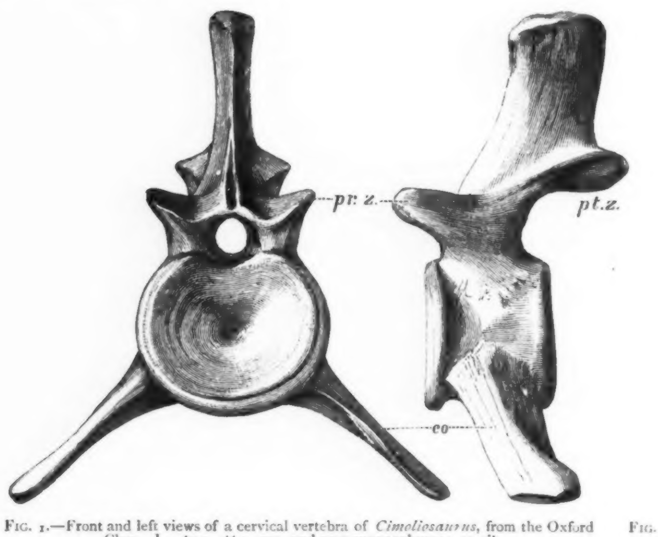

FIG. 1.—Front and left views of a cervical vertebra of *Cimoliosaurus*, from the Oxford Clay. ⅙. *pr.z.*, *pt.z.*, pre- and post-zygapophyses; *co*, rib.

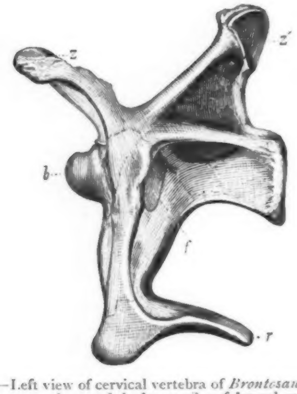

FIG. 2.—Left view of cervical vertebra of *Brontosaurus*. 1/12. *z*, *z'*, zygapophyses; *b*, body; *r*, rib; *f*, lateral aperture.

name with that of the Yorkshire town, we find the author, on p. 670, referring to "Eyebury, near Leeds," and on p. 714, "Eyebury, Yorkshire." Among the so-called Brevirostres we meet with some very interesting, and to us entirely new, information concerning certain small reptiles, from the lithographic limestones of the Continent, known as *Alligatorellus* and *Atoposaurus*. The former is clearly a crocodile, and its resemblance to the latter (which has long been known to possess typical crocodilian feet) points to the conclusion that it should also be regarded as a member of the same order which (like *Metriorhynchus*) had entirely lost its dermal bony armour.

The Dinosaurs, as their importance and interest demand, are treated of very fully. This order, in contravention of some recent proposals, is retained in its widest sense, in which respect we are in full accord with Prof. v. Zittel. It is divided into three sub-orders, for which the terms Sauropoda, Theropoda, and Orthopoda are adopted. The beautiful illustrations (some of which we reproduce) are largely taken from Prof. Marsh's memoirs.

In regard to the vexed question of the relationship of the huge herbivorous forms known as Sauropoda (of which a cervical vertebra is shown in Fig. 2) of Europe and the United States, we certainly do not consider that the author has advanced matters by referring the English *Cetiosaurus* to a distinct family, following this by the American *Atlantosauridæ*, and going on with the *Morosauridæ*, which is taken to include the American *Morosaurus* and the English Wealden forms. *Cetiosaurus* appears to be decidedly inseparable from the *Morosauridæ*, while in the opinion of more than one English writer the Wealden forms should be included in the *Atlantosauridæ*. It is to be feared that the writer of this review is primarily responsible for the relegation of the oldest name *Pelorosaurus* to the rank of a synonym; and it is to be regretted that the name *Ornithopsis* again crops up, after having been shown that it has no title to stand.

In the Theropoda, or carnivorous types, we meet again with the same redundancy of families which cannot possibly be justified. Thus we have *Zanclodontidæ*, *Megalosauridæ*, *Ceratosauridæ*, and *Anchisauridæ;* of which, in our opinion, only two should stand. The reference of *Thecodontosaurus* to the *Zanclodontidæ* cannot for a moment be maintained, since it is a question whether it is even distinct from *Anchisaurus*, while *Zanclodon* appears too close to *Megalosaurus* to be separated as a family. Marsh's figure of the coalesced metatarsals of *Ceratosaurus* is once again reproduced, but, in the face of the note quoted from Dr. Baur to the effect that this is caused by disease, its omission would have been preferable. The author will probably correct the assignation of the name *Epicampodon* to Prof. Huxley. In those remarkable little Dinosaurs known as the *Cœluridæ*, it is unfortunate that Prof. Cope's redetermination of the American forms now known as *Cœlophysis* did not appear in time to prevent their reference to the problematical *Tanystrophæus*. In refusing to regard the European *Compsognathus* and the American *Hallopus* as anything more than the representatives of distinct family groups the author is to be commended. The peculiar backward prolongation of the calcaneum of *Hallopus* (as shown in our reproduction of Prof. Marsh's original figure) is, however, so remarkable as to indicate that its owner is a very aberrant member of the sub-order.

In grouping the Ornithopoda and Stegosauria of Prof. Marsh in a single sub-order, the author follows the lead of Dr. Baur, and the adoption of the name Orthopoda for this group is, perhaps, an improvement on the nomenclature of some other writers. This sub-order is again divided into three sections, the Armoured forms, or Stegosauria, the Horned types (Ceratopsia), and the Iguanodonts (Ornithopoda). In the *Stegosauridæ*, so markedly differentiated by their hoof-like feet (Fig. 4), the author seems to have some doubts as to the identity of the European *Omosaurus* with the American *Stegosaurus;* and he has unfortunately been misled in stating that *O. durobrivensis* is from the Kimmeridge Clay, its real horizon being the Oxfordian. We need say nothing of the wonderful Horned Dinosaurs, as they have been noticed in an article recently published in this journal. In the Iguanodonts the separation of the *Camptosauridæ* from the *Iguanodontidæ* is not supported by any sufficient structural differences; the "pendant" inner trochanter of the femur of *Camptosaurus* being likewise found in some species of *Iguanodon*. In stating, moreover, that *Camptosaurus* differs from *Iguanodon* by the shorter anterior process of its ilium, the author relies on a figure published by Prof. Marsh, which a recent writer states to have been drawn from an imperfect specimen. The disregard of the rights of priority in nomenclature, to which we have already alluded, is again manifest in the retention of the name *Hadrosaurus* instead of the earlier *Trachodon*. In including that most bird-like Dinosaur only recently described under the name of *Ornithomimus*, the work is well up to date.

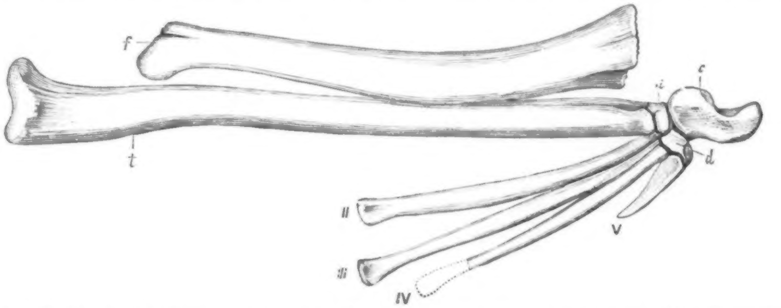

FIG. 3.—Part of left hind limb of *Hallopus*. Nat. size. *t*, tibia; *f*, fibula; *c*, calcaneum; *a*, *d*, distal tarsals; II–V, metatarsals.

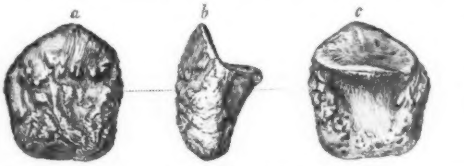

FIG. 4.—Three views of one of the terminal joints of the foot of *Stegosaurus*.

The chapter devoted to the Pterodactyles calls for no special notice; the only innovations being the creation of a distinct family for the reception of the genus *Ornithochirus* of the Cambridge Greensand, and the abolition of the sub-order Pteranodontia, made for the toothless American forms.

The Birds, as their minor palæontological importance demands, have but a comparatively small amount of space allotted to them. They are divided into the three orders Saururæ, Ratitæ, and Carinatæ; the minor groups, usually reckoned as orders by ornithologists, being regarded as sub-orders. The extinct toothed *Hesperornis* is included in the Ratitæ—an arrangement which the author may see reason to revise when he has had the opportunity of studying the memoir on the affinities of this genus recently published by Prof. D'Arcy Thompson. In referring the fragments of Tertiary bones described as *Macrornis* and *Megalornis* provisionally to the Ratitæ, Prof. v. Zittel has been misled by their original description; and he has unfortunately not seen the notice that the so-called *Dromæus sivalensis* was founded upon Mammalian bones. The provisional reference of *Dasornis* to the *Rheidæ* rests on no solid foundation; the genus being apparently allied to *Gastornis*, which is placed—in our opinion incorrectly—among the Carinatæ. The Kiwis and Moas are included in one sub-order; the latter being divided into three genera. If, however, the author were to study carefully the literature of the *Dinornithidæ*, he would find that the name *Palapteryx* cannot be legitimately employed in the sense in which he uses it. There is nothing of special interest to record in the treatment of the fossil Carinatæ.

Throughout the foregoing comments we have endeavoured to find as much fault with the work as we could; the result being that our criticisms are mainly concerned either with small and unimportant points of systematic arrangement, or with slight errors for which the author is frequently not the responsible party. From personal experience we are fully aware of the enormous difficulty of steering clear of errors in a work of this description, and also of arriving at a satisfactory compromise in cases of a conflict of authorities. In both these respects Prof. v. Zittel has passed triumphantly

through the ordeal; and his work may truly be said to be indispensable, not only to the palæontologist, but likewise to every student of zoology who desires to know something more of his subject than can be gleaned from the study of the animals of the present day. R. L.

THE SUNSHINE OF LONDON.

FOR some few years past sunshine recorders have been in operation at four stations situated in various parts of London; and in attempting to gain some idea as to the average duration of sunshine in the metropolis, one is met at the outset by the somewhat perplexing question as to which of these four is best calculated to yield a fair result. One recorder is placed in the heart of the City, at Bunhill Row; and exposed as it is to a maximum amount of smoke and fog there can be little hesitation in saying that its indications are, for the metropolis as a whole, greatly below the mark. Another is stationed somewhat more favourably at Westminster, on the roof of the Meteorological Office, but even there the influence of the surrounding chimneys is felt to a very serious extent, and many a fair winter's day has been known to pass without the registration of so much as a trace of bright sunshine. In a third instance the conditions are reversed, for at the Kew Observatory the air is almost as free from smoke and mist as it is in the open country, so that as a London record the sunshine instrument gives us too high a value. The fourth station appears, however, to be one which strikes a fairly even balance between the meteorological features of the City and those of the more open suburbs; for, although Greenwich is influenced to a greater extent by the impurities of London than Kew, it is sufficiently removed from the central parts of the metropolis to escape much of the fog and smoke which affect the recording instruments both in the City and at Westminster. From a careful examination, we are inclined to think that the Greenwich record supplies a very fair idea of the conditions which prevail over the metropolis as a whole; and as the observations of bright sunshine have now been made at the Royal Observatory for rather over fourteen years, sufficient material has accumulated for the deduction of average results. It is only such sunshine as is strong enough to burn the papers that is here dealt with.

The general results of an examination of the Greenwich records for the fourteen years 1877 to 1890 are given in the following table, which shows for each month, for each season, and for the entire year—firstly, the average number of hours of bright sunshine; secondly, the percentage of the possible amount; thirdly, the average number of hours per day; fourthly, the average amount of sunshine on the brightest day; and fifthly, the number of days on which no bright sunshine was registered. The spring season comprises the months of March, April, and May; the summer those of June, July, and August; the autumn those of September, October, and November; and the winter those of December, January, and February.

From an examination of the table we see that the sunniest month in the year is May, with a total of 179 hours, or 37 per cent. of the possible quantity. June, however, runs it very close with a total of 174 hours, or 35 per cent. of the possible; and, in fact, owing to the slight difference which exists between the length of the two months, the daily average (five hours and three-quarters) is the same in each. It is a somewhat singular fact that the sunniest individual month in the course of the whole fourteen years was neither May nor June, but July. In the July of 1887 the aggregate number of hours recorded was 277, or 56 per cent. of the possible quantity, the daily average being very nearly nine hours. In both May and June we may look for at least one day with brilliant sunshine continuing for about 13 hours, but on an average there are in each month three days on which the solar rays are altogether absent. As regards sunless days, July is the most highly favoured month, for on an average of fourteen years' observations there is then only one day that is continuously overcast. The finest day experienced in the course of the entire period was June 13, 1887, when no less than 15 hours of bright sunshine were recorded.

Months.	Average number of hours of bright sunshine.	Percentage of the possible amount.	Average number of hours of sunshine per day.	Average amount of sunshine on the brightest day.	Number of sunless days.
January	26	10	0·8	5·4	19
February	42	15	1·5	6·9	12
March	95	26	3·0	8·9	6
April	122	29	4·1	11·0	4
May	179	37	5·8	13·0	3
June	174	35	5·8	13·1	3
July	167	34	5·4	12·5	1
August	154	34	5·0	11·6	2
September	115	31	3·8	10·1	3
October	77	23	2·5	7·8	9
November	43	16	1·4	5·8	12
December	20	8	0·7	3·9	20
The Spring	396	31	4·3	13·2	12
The Summer	495	34	5·4	13·2	6
The Autumn	235	24	2·6	10·1	25
The Winter	89	11	1·0	7·0	50
The entire Year ...	1214	27	3·3	13·7	94

The dullest month of the year is December, with a total of only 20 hours of sunshine, or 8 per cent. of the possible, and with 20 sunless days. January is very little better, the total number of hours being then 26, or 10 per cent. of the possible, and with an average of 19 sunless days. In each of these winter months the daily average of sunshine is only about three-quarters of an hour, but after January the weather rapidly improves, February being twice as sunny as its predecessor, and March twice as sunny as February. The dullest month in the course of the whole fourteen years was last December, when the total duration of sunshine was less than two hours and a half, or about 1 per cent. of the possible amount. On 28 days there was a complete absence of sunshine, and of these 28 no fewer than 18 were consecutive. On the brightest December day we can hardly expect four hours of sunshine, but we may certainly look for more than we had last December, when the finest day produced less than an hour and a half.

Turning to the various seasons we find, as we might expect, that the maximum amount of sunshine is recorded in the summer, the average number of hours being 495, or 34 per cent. of the possible amount. The finest summer of the whole fourteen years was the Jubilee season of 1887, when there were 715 hours, or about 50 per cent. of the possible. The dullest summer of the entire series was that of the following year (1888), when the total number of hours was only 373, or 26 per cent. of the possible. The average number of sunless days in the summer months is only six, but in 1888 there were 16, or six more than in any other year of the series. The spring is of course far more sunny than the autumn, the total amount of sunshine in the former season being 396 hours, as against 235 in the latter. In the winter the aggregate amount is only 89 hours, or an average of about one hour per day. The number of sunless days advances from the sunniest to the dullest of the seasons in fairly ap-

proximate geometrical progression, being twice as great in spring as in summer, twice as great in autumn as in spring, and twice as great in winter as in autumn. In a London winter there are more days without sunshine than with it.

The values for the entire year show that the average number of hours of bright sunshine is 1214, or 27 per cent. of the possible amount. The largest number recorded in any year of the fourteen was in 1887, when there were 1407, while the smallest was in that notoriously dismal year 1879, when there were only 984. Taking the year through, the average daily amount of sunshine at Greenwich is little more than three hours and a quarter, or less than half the quantity possible on the shortest December day. Thus, if the sun were to shine all the year through for the same number of hours as the highest possible in mid winter, we should get twice as much bright weather as we actually experience; and the results of an average of fourteen years' observations show that there are 94 days out of the 365, or more than a fourth of the year, upon which the solar rays are either altogether absent or are too feeble to leave any mark on the recording instrument.

FREDK. J. BRODIE.

NOTES.

AT a meeting of the Academy of Sciences held in Paris on Monday, the 2nd inst., Mr. Archibald Geikie, F.R.S., Director-General of the Geological Survey of the United Kingdom, was elected Correspondent of the Institute of France.

ON March 1 a meeting was held in the large hall of the Berlin Rathhaus to do honour to the memory of Dr. Schliemann. The meeting was summoned by the Geographical, the Anthropological, and the Archæological Societies of Berlin. Profs. Virchow and Curtius were the chief speakers, and they eulogized the character and achievements of the famous explorer and archæologist.

THE office of Colonial Bacteriologist at the Cape of Good Hope, has been offered to Dr. Edington of the University of Edinburgh.

REPLYING to Mr. John Ellis in the House of Commons, on Tuesday, Mr. Plunket said that the official guide to the Royal Gardens at Kew, which was at present out of print, was under revision. During the last two years the changes consequent on the rearrangement of the collections had been so extensive, that it had been thought better to suspend the publication of the guide, as in such a period of transition it would mislead, and be a cause of disappointment. Now that the rearrangement of the collections was nearing completion, a new guide had been put in hand, and Mr. Thiselton Dyer hoped to have it ready by next summer.

WE regret to have to record the death of Mr. George Bertin, who was well known as an able and learned student of Assyriology. He died on February 18, in his forty-third year.

PROF. VICTOR HORSLEY will give a discourse at the Royal Institution on "Hydrophobia," on Friday, March 20, in place of Prof. W. E. Ayrton, who is unable at present to give his promised lecture on "Electric Meters, Motors, and Money Matters."

ON Friday last, Mr. Goschen received a numerous deputation representing the University Colleges of England, who presented a memorial in favour of the annual grant, which is now £15,000, being increased. The deputation was introduced by Mr. Chamberlain, who, with Mr. Mundella, Sir Henry Roscoe, and others, strongly supported the memorial. Mr. Goschen, in the course of his reply, said they would not expect him without consultation with his colleagues, or at once after the reception of a deputation such as that, to give any final answer, but he was disposed to admit that in many ways they had made out a strong case. At all events they had made out this case, that the Colleges had been doing an increased good work, and that they thought that with additional sums they would be able to widen still the area of their usefulness. One point had struck him while the discussion was going on—and he might some time discuss it with Mr. Chamberlain, who would be a good authority on the question—namely, whether, instead of relations being established between the Imperial Government and these Colleges, it would not be better that the tie should exist between the County Councils and the Colleges, which would stimulate local interest. That was a matter which was too wide for discussion then; but he threw out the idea, in case it should become at any time the subject of their future consideration. It might be that the County Councils would throw their hearts into this work, as he believed they were doing in many parts of the country into the question of technical and other education, and for his part he should see no objection to any course that would tend to increase local interest in these Colleges.

IN several journals attention has lately been called to the fact that for women there exists at the present moment at Bedford College the very facilities for study which University College and King's College propose to offer when the present scheme for the development of their scientific teaching is carried out. Bedford College, one of the earliest established for women only, was founded as long ago as 1849; and when the University of London admitted women to its degrees, Bedford College students were the first to graduate there, while of those women who have since taken degrees in arts and science, a large proportion have belonged to the same institution. A new wing has just been added to the College, so that four separate laboratories are now open daily for the use of women desirous of carrying on practical work in biology, botany, chemistry, or physics. Special advantages are provided for those who have any bent for higher work or for original research. As the public opening of the new wing has been unavoidably postponed, the Council of the College desire it to be known that since Easter, 1890, the new laboratories have been in full working order, and that they can now offer accommodation to a greatly increased number of women students.

AT a meeting of the Biological Society of Washington, on February 7, Mr. Charles D. Walcott, of the U.S. Geological Survey, announced the discovery of vertebrate life in the Lower Silurian (Ordovician) strata. He stated that "the remains were found in a sandstone resting on the pre-Palæozoic rocks of the eastern front of the Rocky Mountains, near Canon City, Colorado. They consist of an immense number of separate plates of placoganoid fishes, and many fragments of the calcified covering of the notochord of a form provisionally referred to the Elasmobranchii. The accompanying invertebrate fauna has the facies of the Trenton fauna of New York and the Mississippi valley. It extends into the superjacent limestone, and at a horizon 180 feet above the fish beds seventeen out of thirty-three species that have been distinguished are identical with species occurring in the Trenton limestone of Wisconsin and New York. Great interest centres about this discovery from the fact that we now have some of the ancestors of the great group of placoderm fishes which appear so suddenly at the close of the Upper Silurian and in the lower portion of the Devonian groups. It also carries the vertebrate fauna far back into the Silurian, and indicates that the differentiation between the invertebrate and vertebrate types probably occurred in Cambrian time." Mr. Walcott is preparing a full description of the stratigraphic section, mode of occurrence and character of the invertebrate and vertebrate faunas, for presentation at the meeting of the Geological Society of America, in August next.

A Society for the Study of the Flora of France has been instituted by M. Camus, of Paris, and M. Magnier, of Saint-Quentin (Aisne), somewhat on the lines of our Botanical Exchange Club. Its special object is the distribution of authentic specimens of rare and critical species, remarkable varieties, and hybrids.

The State of Dakota has established a Botanical Experiment Station at Fargo, under the charge of Mr. Henry L. Bolley, of the Indiana Experiment Station.

We learn from the *Botanical Gazette* that Dr. J. T. Rothrock had arranged a biological expedition to the West Indies and Yucatan, and was to spend the months of November, December, and January in those countries. The party was provided with an excellent ship with abundant storage room. Mr. A. S. Hitchcock was to accompany the expedition in the interests of the Missouri Botanical Garden.

The fifth Annual Report of the Ornithological Observation Stations in the kingdom of Saxony has been issued. It is edited by Dr. A. B. Meyer and Dr. F. Helm. The Report records the observations made in the year 1889 by 47 observers at 45 stations, on 209 species of birds.

Mr. G. J. Symons, F.R.S., writing to the *Times*, from 62 Camden Square, N., speaks of the extraordinary dryness of the month of February. He says:—"My observations here have been absolutely continuous for more than thirty years, and hitherto the driest February was that of 1863, when 0·31 inch fell. In 1891 we have less than one-thirtieth of that; we have only 0·01 inch. And if we examine all the other months of the whole thirty-three years we find that the driest was May 1885, with 0·26 inch. These two facts sufficiently indicate the exceptional character of the past month at this station. We had one slight sprinkle in the forenoon of February 7, immediately after one of those intense darknesses (arising from high fog) which are becoming so sadly more frequent in this wilderness of chimneys. It had been dark—actually darker than on a clear moonless night. Fine mist began to fall. I put some sheets of note paper in the garden for the rain to fall upon. The shower, if such it could be called, was over in an hour, and every drop had left its inky mark upon the paper. I inclose a portion that you may have one more proof of the need for drastic measures if London is to be clean enough to live in." Mr. Symons has received only one return from England exceeding 0·10 inch, and this was from the hills above Ullswater.

A Bill introduced into the House of Lords by Lord Stratheden and Campbell for the abatement of the smoke nuisance in London was read a second time on Monday, and afterwards referred to a Committee. On the same evening, in the House of Commons, Viscount Wolmer asked the First Lord of the Treasury whether, considering the serious injury to health, the disturbance of business, and the hardships inflicted on many of the poorest wage-earners of the metropolis by the curtailment of their working hours, caused by the increasing prevalence of fogs, Her Majesty's Government would consider the advisability of appointing a Royal Commission to examine and report how far the evil was one which could be mitigated by legislation. Mr. W. H. Smith replied:—"I have to assure my noble friend that Her Majesty's Government are, in common with other inhabitants of the metropolis, extremely sensible of the serious injury, disturbance, and hardship inflicted by the increasing prevalence of fog. They are, however, sceptical of the value of a Royal Commission to investigate the subject. It is notorious that the evil mainly arises from the smoke emitted by ordinary domestic fires, and the problem to be solved is, whether it is possible by legislation to prohibit and prevent the production of smoke in this way. A Bill was introduced into the House of Lords in 1887 with this object, and was referred, after a second reading, to a Select Committee, which took evidence that went to show that smoke could be prevented by the use of non-bituminous coal, by the substitution of coke or gas for coal for heating purposes, and possibly by the adoption of an improved grate, and by great care in the lighting and feeding of fires in the metropolis. The Bill was not proceeded with beyond the Committee stage of 1887. Another Bill has been introduced with the same object, and stands for second reading in the House of Lords this evening. If it should come down to this House, the noble lord will have an opportunity of considering whether it is possible by penalty on occupiers of houses and tenements to secure the object he has in view." Sir H. Tyler had a question on the subject of an inquiry into the various descriptions of coal used in the metropolis. Mr. W. H. Smith said the matter had already had attention from a Committee of the House of Lords, and he doubted whether it would be possible to carry it further.

Ciel et Terre of February 16 contains an article by A. Lancaster, of the Brussels Observatory, on the severe frosts of this century. He has examined the statistics with the view of finding some clue to the probable future weather, and finds: (1) that a cold winter has never been followed by a very hot summer; and (2) that in the great majority of cases the summer following a severe winter has been cold. The same opinion has been expressed by Humboldt in his *Cosmos*, and by others. M. Lancaster concludes from the comparisons he has made that there is a great probability of a cold summer this year, and that June and July particularly will have a low temperature. Further, that as every prolonged period of cold in summer coincides with rainy weather, we may expect a wet summer. In support of this opinion he quotes the fact that out of fifteen cold winters between 1833-90 all but two have been followed by wet summers.

The Government have chartered the steam yacht *Harlequin*, 160 tons, for fishing-experiments off the Irish coast. She will be in commission three months. She has a powerful electric search light, and her steam appliances include a winch capable of lifting a 7-ton trawl.

A telegram from Washington says it has been decided that the Arctic Expedition under the command of Civil Engineer Peary, of the United States Navy, will leave Inglefield Gulf, on the west coast of Greenland, and take a north-easterly course by sledges. The party will consist of picked men, and will start from New Bedford, Massachusetts, on May 1. The scientific and geographical Societies of the United States will defray the expenses.

The Journal of the Camera Club, vol. iv., Nos. 40-52, for the year 1890, contains many articles that will be read with interest by photographers, both amateur and professional. Among them we notice those on the density of negatives, on pin-holes, &c., by Captain Abney; limitations in the treatment of subjects, practical interpretation of the law of conjugate foci, by T. R. Dallmeyer; eclipse photography, by A. A. Common; photography by the light of the electric spark, by Lord Rayleigh. Besides these, there are many notes, reviews of books, and accounts of excursions made to various places of note for the purposes of photography, and several other items of photographic news. Illustrations of the new premises that are being specially built for the Club are given, showing plans of the various floors, and from them we see that ample provision is being made for the requirements of photography in the form of dark rooms, studios, enlarging rooms, and rooms for meeting. In this volume, also, is inserted a very pretty illustration, entitled "Twitch Burning," the print being by the Praphstone Company, Limited.

IT is commonly believed that the aboriginal Aino race, inhabiting the Island of Yezo in Northern Japan, is rapidly dwindling away before the advance of civilization. But, according to the information lately published in Japan, this popular belief does not appear to be altogether corroborated by facts. Under the old Japanese *régime*, the population of Yezo emigrants from the other Japanese islands was estimated at 40,000, but in consequence of protection and encouragement given by the reformed Government, this number increased to 350,000 in 1888. As a natural consequence, the Ainos' means of sustenance were encroached upon, and they have been popularly believed to be going down before the advance of the dominant race. But the latest statistics show that the truth is far from the common hypothesis, and that this curious and interesting race is not dying out, as has been supposed. The exact figures are as follows:—

Aino Population in Yezo.

Year.	Male.	Female.	Total.
1872	7964	7311	15,275
1873	8167	8032	16,199
1874	8171	8160	16,331
1875	8547	8583	17,130
1876	8579	8598	17,177
1877	8483	8483	16 966
1878	8537	8521	17,058
1879	8513	8515	17,028
1880	8566	8575	17,141
1881	8476	8457	16,933
1882	8546	8652	17,198
1883	8617	8615	17,232
1884	8702	8770	17,472
1885	8635	8687	17,322
1886	8464	8571	17,035
1887	8437	8525	16,962
1888	8475	8587	17,062

MESSRS. CASSELL have published the first part of what promises to be a book of considerable value. The work is entitled "Our Own Country," and will present, with illustrations, a geographical and historical description of the chief places of interest in Great Britain and Ireland. The same publishers have just issued the first part of a work on "Familiar Trees," by G. S. Boulger, with 80 coloured plates; and Part 29 of their excellent "New Popular Educator."

A WORK entitled "Memorials of John Gunn" will shortly be issued. It will be edited by Mr. Horace B. Woodward, and will contain notes by the late Mr. Gunn, presenting some account of the Cromer forest bed and its fossil Mammalia, and of the associated strata in the cliffs of Norfolk and Suffolk. Mr. Gunn, while acting as a country clergyman, had for many years devoted a large share of his attention to the geology of Norfolk. The "Memorials" will be accompanied by a portrait and biographical sketch of the author.

THE following lectures will be given at the Royal Victoria Hall: March 10, Mr. J. E. Flower on "Spain"; 17th, Mr. A. W. Clayden on "Thunderstorms"; 24th, Prof. Roger Smith on "St. Paul's."

PROF. SEUBERT contributes an important paper to the current number of *Liebig's Annalen*, in which are presented the final results of his redetermination of the atomic weight of osmium. A preliminary account of the earlier portion of this work was published in the *Berichte* in June 1888, and a short notice concerning it was given in these columns (vol. xxxviii. p. 183). It was then shown that the atomic weight of osmium was certainly not higher than 191, and was probably a few decimals less. Owing, however, to lack of material, Prof. Seubert was not able to complete the work in the unimpeachable manner characteristic of his other atomic weight determinations. Since that time, however, thanks to the kindness of Prof. Lothar Meyer, a sufficient quantity of pure osmium has been placed at his disposal, and the work has been completed in a manner which leaves nothing to be desired. The salts analyzed were potassium and ammonium osmium chloride, K_2OsCl_6 and $(NH_4)_2OsCl_6$. The final mean value derived from all the experiments is 190·3, a number which fully justifies the expectations of Prof. Seubert that it would fall slightly below 191. The importance of the settlement of this question cannot be over-rated, for it removes the last outstanding exception to the periodic generalization. The metals of the platinum group—osmium, iridium, platinum, and gold—when arranged in the order of their chemical and physical properties unmistakably take the relative precedence just quoted. If these properties are, as everyone now agrees, periodic functions of atomic weight, the atomic weights of these metals should increase from that of osmium upwards to that of gold. Previous to the year 1878, however, the accepted atomic weights were: gold 196·2, iridium 196·7, platinum 196·7, and osmium 198·6—a relation which, if correct, was diametrically opposed to the principle of periodicity. In that year Seubert attacked the subject, and the first outcome of his labours was to correct the atomic weight of iridium, which he found to be 192·5, instead of 196·7. It was a most remarkable tribute to the accuracy of Seubert's work, and likewise of his own, that Joly a short time ago obtained for the same constant the identical number, 192·5. In 1881, Seubert took up the case of platinum, and finally adjusted its atomic weight to 194·3—a number which was confirmed by a subsequent determination of Halberstadt. In 1887 the position of gold was finally decided by the remarkably agreeing and almost simultaneous determinations of Thorpe and Laurie on the one hand, and Krüss on the other, the value arrived at in both cases being practically 196·7. Finally, we have the just completed work of Seubert upon osmium; and the four metals, when arranged in order of atomic weight, now take the order: osmium 190·3, iridium, 192·5, platinum 194·3, gold 196·7—an order of precedence in full accord with the order of their chemical and physical properties.

THE additions to the Zoological Society's Gardens during the past week include a Red-throated Diver (*Colymbus septentrionalis*), British, presented by Mr. E. J. Gale; a Herring Gull (*Larus argentatus*), British, presented by the Rev. C. A. Berry; twelve Grayling (*Thymallus vulgaris*) from British fresh-waters, presented by Messrs. Howard L. Cooper and — Jukes; a Wapiti Deer (*Cervus canadensis* ♂) from North America, a Long-tailed Weaver Bird (*Chera progne*) from South Africa, two Chinese White-Eyes (*Zosterops simplex*) from China, deposited; four Upland Geese (*Bernicla magellanica* ♂ ♂ ♀ ♀) from the Falkland Islands, purchased; a New Zealand Parrakeet (*Cyanorhamphus novæ-zealandiæ*) from New Zealand, two Wonga-Wonga Pigeons (*Leucosarcia picata*) from New South Wales, received in exchange; a Gayal (*Bibos frontalis*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

THE PERMANENCE OF MARKINGS ON VENUS.—In *Bulletin No. 12 de l'Académie Royale des Sciences de Belgique*, Dr. Terby contributes a paper entitled, "Facts demonstrating the permanence of the dark spots on Venus, and the slowness of their motion of rotation." Dr. Terby made a series of observations of Venus between April and August 1887, and sent some of the results, in a sealed packet, to the Academy in the same year. A similar series of observations was made by M. Perrotin from May to September 1890, and the results were communicated to the Paris Academy (*Comptes rendus*, October 27, 1890). The object of the present paper is to call attention to the fact that the drawing made by M. Perrotin in 1890 agrees in every respect with those made by Dr. Terby in 1887. Each observer made drawings of two types of markings, and in each case the change from one type to the other took place about two months

after the first observation. A comparison shows that Venus occupied very nearly the same position in her orbit during the time that the drawings were made by the two observers. The earth's position was also little different in both cases. This agreement between independent observers seems therefore to justify the following conclusions:—

(1) The observations were made on the same portion of the planet's surface.

(2) Venus, in travelling over the same part of her orbit, after an interval of three years, or after five complete revolutions, presented the same part of her surface to the same part of the heavens during the respective periods of observation.

(3) Therefore the planet at these two epochs also presented very nearly the same face to the sun.

(4) Venus has therefore a very slow rotational motion, as has been suggested by Signor Schiaparelli, and confirmed by M. Perrotin.

(5) There exist not only light spots on the planet's surface, but also dark ones having a fixed character similar to those on Mars; these spots are fixed enough to be recognized after an interval of three years, but are difficult to see, on account of their undefined edges, due, probably, to a dense atmosphere full of clouds. Up to now, the permanence of the dark spots was much questioned; these spots, however, do not appear to resemble those drawn by Bianchini and De Vico.

Observations made by various other observers are quoted in support of these conclusions, and in favour of the probability that the time of revolution is equal to that of rotation. A plate containing forty-three drawings of Venus accompanies the paper.

OBSERVATIONS OF MARS.—The above-named *Bulletin* contains also some observations of Mars made by M. J. Guillaume last year, by means of a With's reflector about 9 inches in aperture, and a power of about 195. Twenty-five drawings of the planet are given, and with each the times of observation and the longitude of the central meridian calculated from the ephemerides furnished by Mr. Marth. This facilitates the comparison of the drawings with those made by other observers. In spite of the bad atmospheric conditions under which M. Guillaume, like many other astronomers, has had to labour, he has had the good fortune to see a certain number of canals, and gives a drawing of their gemination. The variations in the appearance of some of the seas observed on different occasions leads to the conviction that they can only be explained by the presence of clouds in the Martian atmosphere.

NEW VARIABLE.—*Wolsingham Observatory Circular*, No. 29, reads:—A star, 8·5 mag., red, third-type spectrum, not in the D.M., was observed here on January 31 and February 1 and 2, in R.A. 5h. 32m. 37s., Decl. + 31° 57′ (1855). The star is perhaps variable.

NEW ASTEROID.—M. Charlois, of Nice Observatory, discovered another asteroid on February 16. Its designation will be (306); its magnitude is 11·5.

THE MAMMALIAN NERVOUS SYSTEM.[1]

1. *Introduction.*

IN the Proceedings of the Royal Society, No. 273, November 1888 (vol. xlv., 1889, p. 18), we published a preliminary account of some of the experiments of which the results are given in detail in our full paper.

In that communication we stated that the object of our work then was to endeavour to ascertain the character of the excitatory processes occurring in nerve-fibres when either directly, *i.e.* artificially excited, or when in that state of functional activity which is due to the passages of impulses along them from the central apparatus. The most important way in which such a method could be applied was, obviously, one which would involve the investigation of the excitatory changes occurring in the fibres of the spinal cord when the cortex cerebri is stimulated. We must at once assume that the motor side of the central nervous system is practically divisible into three elements:—(1) *Cortical centres.* (2) *Efferent* (pyramidal tract) *fibres*, leading down through the internal capsule, corona radiata, and spinal cord. (3) *Bulbo-spinal centres* contained in the medulla and the spinal cord, and forming the well-known nuclei of the cranial and also of the spinal motor nerves.

It had already been determined, both by direct observation and by the graphic method, (1) that certain areas of the cortex were connected with definite movements of various parts of the body, and (2) that while the complete discharge of the cortical apparatus was followed by a very definite and characteristic series of contractions of the muscles in special relation with the particular point excited, the effectual removal of the cortical central mechanism and subsequent excitation of the white fibres passing down through the internal capsule, &c., led to the production of only a portion of the effect previously obtained from the uninjured brain.

This method of observation in no wise showed what processes were actually occurring in the spinal and other nerve-fibres, and although the ablation of the cortical centre to a certain degree suggested the extent to which the cortex acted, nevertheless it did not afford an exact demonstration of the same. Moreover, the data which the graphic method furnished were precluded, through their being muscular records, from determining what share, if any, the lower bulbo-spinal central nerve-cells took, either in the production of the characteristic sequence of contractions, or in the modification, whether in quality or in force, of the descending nerve-impulses during their transit. It seemed to us that the only way to approach this subject would be to get, as it were, between the cortex and the bulbo-spinal system of centres. This would be accomplished if some means were devised of ascertaining the character of the excitatory processes occurring in the spinal fibres of the pyramidal tract when, upon excitation of the cortex, nervous impulses were discharged from cortical cells, and travelled down the cord.

The question as to the extent to which it is possible to obtain physical evidence of the actual presence in nerve-fibres of excitatory processes, and thus to arrive at trustworthy data for the comparison of their amounts, is one which, up to the present, has been answered only indirectly, and that in two ways: first, by the extension of Helmholtz's classical experiment of determining the rate of transmission; and secondly, by observing those variations in the electrical state of nerve-fibres which Du Bois-Reymond discovered to be an invariable concomitant of the excitatory state. As will subsequently be shown in the historical retrospect, it is well known, through the researches of Du Bois-Reymond and others, that the fibres of the spinal cord, just as nerve-fibres in the peripheral trunks, are characterized by showing, when unexcited, an electrical difference between their longitudinal surface and cross-sections; and, furthermore, that when excited, a well-marked diminution of this resisting electrical state is produced in the fibres of the cord, as in those of nerve-trunks. Now, since such excitatory variations in the electrical state are presumably parallel in time and amount with the presence in the nerve of the series of unknown processes, termed excitatory, which a series of stimuli evokes, it was reasonable to presume that, if the cortex were discharging a series of nerve-impulses at a certain rate down the pyramidal tract, there would be a series of parallel changes in the electrical condition of the fibres in the cord tract, and that, with a suitable apparatus for responding to such changes, these might be both ascertained and recorded. The accomplishment of a further purpose, viz. the localization of both paths and centres by ascertaining the excitatory electrical effects in relation with them, was one of the main objects we had in view. In carrying it out, we found it was unnecessary to employ the electrometer, and, in fact, that it was advantageous to use the galvanometer, the record of which would be more easily and more accurately noted, since its graduation admits of far higher magnification. Moreover, with this instrument it was possible, by employing a series of stimuli, of known number and duration, to obtain quantitative results of definite comparative value, as will be shown further on; and thus, to compare both the size of different central paths and the amount of nervous energy discharged along the same path from different sources.

The plan upon which the full paper is framed is, first, to give an historical retrospect of the work of authors who have opened up the study of electrical changes in the central and peripheral nervous system; second, to describe at length our mode of experimentation, with special reference to the modifications which we have introduced; then to compare roughly

[1] "On the Mammalian Nervous System; its Functions and their Localization determined by an Electrical Method." By Francis Gotch, Hon. M.A., Oxford, and Victor Horsley, F.R.S. (From the Physiological Laboratory of the University of Oxford.) Croonian Lecture read before the Royal Society on February 26.

the results we have obtained by our present method with those which had been previously ascertained by the graphic method, and so introduce the description of the facts which we have discovered, elucidating the physiology of the spinal cord, both in its relation to the higher centres, and to the peripheral nerves.

2. *Experimental Procedure.*

The observations were in all cases made on etherized animals (cat and monkey), with due regard to the special influence of the anæsthetic. The operative procedure was so designed as to provide for suitable exposure of a particular region of the nervous system for excitation, and of another part in which the electromotive changes evoked by the stimulation may be observed. The relative parts were as follows:—

Part exposed for excitation.	Part exposed for observation.
Brain (cortex and corona radiata) ...	and spinal cord.
,, ,, ,, ...	and mixed nerve.
Spinal cord	and spinal cord.
,, ...	and mixed nerve.
Mixed nerve...	and spinal cord.
Spinal roots	,, ,,
Posterior roots	and mixed nerve.

The excitation was either electrical, chemical (*i.e.* with absinthe and strychnine), or mechanical. In the former instance the duration and intensity were specially determined. The records were made by a Thomson high-resistance reflecting galvanometer and a Lippmann's mercurial capillary electrometer.

The tissue, whether nerve or spinal cord, was so arranged for observation as to be always suspended in the air, one end remaining in connection with the animal; consequently any error due to current derivations from the rest of the body could only have a slight and unipolar effect.

3. *Resting Electrical Difference between the Cut Surface of the Tissue and its Uninjured Longitudinal Surface.*

The average amounts of this difference in the tissues observed were as follows:—

	Cat.		Monkey.
Nerve (69 cases),	0·01 Daniell ...	(12 cases),	0·005 Daniell.
Root (5 cases),	0·025 ,, ...		—
Cord (50 cases),	0·032 ,, ...	(9 cases),	0·022 ,,

We have observed that the cord difference is greater when that tissue is in connection with the higher centres, and that it rises after each excitation. An important fall of the difference is to be remarked in all three tissues as a direct result of systemic death.

4. *Electrical Changes in the Spinal Cord evoked by Excitation of the Cortex Cerebri and Corona Radiata.*

We further discuss in our full paper the following points additional to those described in our previous communication, and which have resulted from the observation of the above changes:—

(*a*) Localization of cortical areas of representation in relation to the various regions of the cord.

(*b*) Bilaterality of representation in the central nervous system, as evidenced by the electrical changes in the two halves of the spinal cord, consequent upon excitation of the brain or cord.

5. *Electrical Changes in the Spinal Cord when evoked by Direct Excitation of its Fibres, after Severance from the Encephalon.*

We have by employment of this method ascertained the proportionate existence of direct channels in the various columns of the spinal cord, our design embracing the quantitative comparison of the electrical changes (and so indirectly of the nerve-impulses) which are transmitted as a result of minimal excitation of the fibres. To further control our observations on these points, we have also determined the extent of interruption in any given channel by intervening sections of the same.

As an extension of this subject, we have investigated the concurrent spread of nervous impulses to collateral paths, and probably to centres, when this further condition is introduced by increase in the stimulus.

The above results have been obtained in the case of both ascending and descending impulses.

Among other general conclusions from this division of our research are the following:—

(1) High degree of unilaterality of representation in the spinal cord.

(2) Spread of impulses from one posterior column to another and from one posterior column to its neighbouring lateral column through centres.

6. *The Relation of the Paths and of the Bulbo-Spinal Centres in the Spinal Cord to the Peripheral Nerves and their Roots.*

We have investigated this important relationship in the following modes:—

(I.) *The Electrical Changes in the Spinal Cord evoked by Excitation of a Mixed Nerve or its Roots.*—The chief conclusions which have been deduced from the results of these experiments, by means of minimal excitation and the employment of the method of blocking by intervening sections, include the following:—

(1) Complete obstruction offered to centripetal impulses reaching the cord by the central end of the anterior root.

(2) Mode of conduction, direct and indirect, in the cord of centripetal impulses passing up the posterior root.

(3) Localization of the direct path of afferent impulses in the posterior column of the same side as that of the nerve or root excited.

(4) Localization of the indirect path of afferent impulses in the posterior columns of the same and the opposite side and the lateral column of the same side as that of the nerve excited.

(5) Proportionate development of both systems of paths in the two sides of the cord.

Expressed in percentages of the total transmission, this proportion is as follows:—

Posterior column of same side as the excited nerve ...	60 p.c.
Lateral column of same side as the excited nerve ...	20 ,,
Posterior column of opposite side to the excited nerve	15 ,,
Lateral column of opposite side to the excited nerve...	5 ,,

(II.) *The Electrical Changes in a Mixed Nerve or its Roots evoked by Excitation of the Spinal Cord.*—Whereas in the foregoing series (I.) we dealt only with ascending impulses, we proceeded to investigate the distribution of descending impulses by observing the above-named changes when the individual columns of the cord are excited by minimal and later with more intense stimuli, controlling our results by the method of intervening sections.

We can summarize the effects observed as follows:—

(1) Marked quantitative diminution suffered by impulses leaving the spinal cord by the anterior roots, whether originating in the cortex cerebri, corona radiata, or the lateral columns of the cord.

(2) Localization of direct transmission of impulses in the posterior column and passing out into the posterior roots of the same side.

(3) Proportionate development of the direct and indirect paths in the individual columns of the cord, passing out into the mixed nerve of the one side.

(4) Effects observed in the posterior roots when the bulbo-spinal centres are excited either by strychnine or electrically (kinæsthesis).

Finally, the chief general principles elucidated by this research may be stated as follows:—

(1) Unilateral character of the representation of function in the paths of the central nervous system.

(2) The physiological characteristics of the regions of a nerve centre:—

(*a*) The kinæsthetic activity of the afferent region of the centre.

(*b*) The obstruction offered by the efferent region, including the field of conjunction, to the transmission of impulses through the centre.

THE CITY AND GUILDS OF LONDON INSTITUTE.

AN important memorandum on the examination, inspection, and payment of grants in aid of local technological and trade classes has been issued by the City and Guilds of London Institute to County Councils and municipal bodies. So many changes are likely to be effected by means of the funds which

have lately been granted to local authorities for the promotion of technical education, that it was necessary for the Institute to consider the various ways in which it might hope to meet most effectually the altered conditions. In the following statement it has summed up clearly the main facts of the situation :—

(1) Recent legislation has placed funds at the disposal of County Councils for the purposes of technical education, and has imposed on local authorities the responsibility of determining the best means of utilizing those funds in accordance with the provisions of the Technical Instruction Act.

(2) Whilst the Science and Art Department pays grants on results on all subjects of instruction included in its Directory, no such payments are made by the Department on the technological and trade subjects included in the Institute's programme. Hitherto, grants on such subjects have been paid by the Institute, but these grants will be discontinued after the year 1892.

(3) The Institute is prepared to continue, and to defray the cost of its present system of technological examinations, and to improve it from time to time—

(*a*) By the addition of further practical tests.

(*b*) By adapting the examinations to a still greater extent to local requirements and to the different sections into which many trades are now divided.

(*c*) By the addition of new subjects of examination.

(4) The administration of the present system of examinations involves—

(*a*) The organization of technical classes adapted to different trades.

(*b*) The selection and appointment of competent examiners and local superintendents.

(*c*) Inquiry into the qualifications, and the registration, of teachers.

(*d*) The preparation of syllabuses of instruction and examination.

(*e*) The recommendation, of books of reference, for the use of students and for the formation of technical libraries in connection with separate trades.

(*f*) The examination of registers of attendance of students at classes, and of certificates as to their occupations.

(*g*) The award of different grades of certificates, of silver and bronze medals, and of the money prizes, provided by the separate Livery Companies of London.

(5) The present system of examinations would be further improved by the inspection of classes by competent experts in different branches of trade, and by other persons possessing the necessary scientific knowledge and educational experience. Information might thus be obtained as to the efficiency of the teaching, the provision made for practical instruction in the laboratory or workshop, and the adequacy of the equipment as regards apparatus, models, and machinery. The Institute has under consideration the expediency of organizing such a system of inspection.

(6) The Institute would be prepared to submit to County and Borough Councils co-operating with it, reports on the attendance of students at technological classes, and on the results of the examination of such students, which would serve to guide County Councils in making grants in aid of the maintenance of such classes.

(7) It is suggested that the grants in aid might be of two kinds :—

(*a*) Capitation grants on students in regular attendance at classes certified by the Institute.

(*b*) Grants on the results of the Institute's technological examinations.

(8) No grants have been hitherto given under section (*a*), although the want of such assistance has been generally felt and fully recognized. The grants under section (*b*), given by the Institute, consist of £2, and £1, on behalf of every candidate who takes a first-class or second-class certificate, and is actually engaged in the trade connected with the subject of examination.

(9) The Institute possesses the administrative machinery required for furnishing local authorities with the necessary detailed reports for the award of grants under each of the above sections, and is prepared to place its technical and educational experience at the service of County and Borough Councils for the purpose hereinbefore indicated.

(10) The Institute would be further prepared to make arrangements with County or Borough Councils co-operating with it, for submitting reports on the general efficiency of technological classes, and for affording assistance in the organization of new schools or classes for the promotion of technical education in connection with local trades and industries.

These suggestions are made with the view of indicating the position which the Institute has occupied during the last twelve years, and that which it is prepared to occupy in future, in relation to technical education throughout the country, and of enabling County and Borough Councils, if they so desire, to avail themselves of the assistance thus offered in improving, and in subsidizing, by the payment of grants, the technological and trade classes now established in the chief centres of industry, many of which, it is feared, without such help, will be unable to continue to exist.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

Cambridge.—Sir Alfred C. Lyall, K.C.B., K.C.I.E., has been appointed Rede Lecturer. His subject is "Natural Religion in India."

Prof. Roy announces for the Long Vacation a course of instruction in "Bacteriology," suitable for candidates for the University diploma in public health. Dr. A. Gamgee delivers in this term and the next a course in pathological chemistry.

The Special Board for Medicine have issued revised schedules defining the range of the examinations in physics and chemistry for the M.B. degree. Johns Hopkins University is recognized by the Board as a school of medicine.

SOCIETIES AND ACADEMIES.

London.

Royal Society, February 12.—"On the Demonstration by Staining of the Pathogenic Fungus of Malaria, its Artificial Cultivation, and the Results of Inoculation of the Same." By Surgeon J. Fenton Evans, M.B. Communicated by Prof. Victor Horsley, F.R.S. (From the Laboratory of the Brown Institution.)

The discovery of organisms constantly concomitant with manifestations of malaria was made by Laveran in 1880.

His researches have since been corroborated and amplified by numerous observers in different parts of the world, among whom must be mentioned, Marchiafava Celli, Golgi, and Guarnieri, in Italy ; Councilman, Osler, and James, in America : and Vandyke Carter, in India. The foreign structures which all of the above-named investigators agree in finding in the blood during or after attacks of ague may be grouped into the following classes :—

(1) "Cystic" bodies or spores, 2 to 11 μ in diameter, round, transparent, encapsuled bodies of variable dimensions.

(2) Crescentic bodies, 8 to 9 μ long and 3 μ broad.

(3) Plasmodia malariæ, organisms as variable in size as the "cystic" bodies or spores, possessing the power of amœboid movement, and so closely associated with the red blood corpuscle that hitherto the majority of observers have considered them to be parasites situated within the red blood cells.

(4) Mobile filaments, 21 to 28 μ long.

Despite the general concord of the observations, the subject has not advanced beyond the stage of recognition of these structures in the blood, and that, too, only while in the fresh state.

No method had hitherto been discovered of preparing permanently stained specimens of the organism.

It had never been isolated or classified, nor when thus separated had its pathogenic qualities ever been tested by experiments on lower animals.

It was thus clear that much remained to be done, and in the paper are recounted the attempts made to place the subject on a satisfactory footing. The author has found that it is possible to stain the organisms with an anilinized alkalized solution of rosanilin hydrochloride after treatment with bichromate of potash, and after treatment with dilute sulphuric acid by an anilinized alkalized solution of Weigert's acid fuchsin.

Another method of staining consisted in the saturation of the tissue with a copper salt and its reduction by sulphuretted hydrogen previous to coloration with anilinized alkalized acid fuchsin.

By these staining methods the organisms have been demonstrated in the blood, and also in the tissues. And some new, hitherto unrecognized features are described, among which may

be mentioned what appears to be the germination of the spore in the blood, the existence of a comma-shaped body and of mycelium in the spleen and Peyer's glands, and the localization of the plasmode, *i.e.* in relation to the blood corpuscles.

The isolation of the organism and its artificial cultivation have been successfully carried out, and it is shown that this result entirely depends for its success upon the fact that the nutrient media must be previously treated with living blood, *i.e.* before *rigor mortis* has set in.

Alteration in the chemical composition of the nutrient medium, consisting in the addition of glucose, together with iron or hæmoglobin or fresh blood, to the non-peptonized beef broth, elicited the interesting fact that, under these circumstances, the organism can pass to a more highly developed state, displaying the structure and fructification of a highly organized fungus, but differing in certain important features from any fungus hitherto described.

Inoculation of guinea-pigs, monkeys, and rabbits with the growths in various nutrient media has produced a frequently fatal disease, which, although not characterized in these animals by the symptoms of classical intermittent fever, yet displayed in a number of instances a definitely intermittent character. It was further, whatever its clinical character, invariably accompanied by the appearance of the characteristic organisms in the blood drawn after death from the right ventricle.

It is accordingly concluded that the malarial fungus is capable of being cultivated outside the body, and has been proved to possess pathogenic qualities.

Zoological Society, February 17.—Prof. Flower, F.R.S., President, in the chair.—Mr. Edward Gerrard, Jun., exhibited an extraordinarily large head of a Koodoo Antelope (*Strepsiceros kudu*), which had been shot by Mr. F. C. Selous, near the river Macloutsie, Khama's Country, South Africa, in May last.—Mr. T. D. A. Cockerell exhibited and made remarks on a curious and rather noteworthy monstrosity of a Land-shell (*Clausilia rugosa*) with two apertures.—Mr. G. A. Boulenger exhibited and made remarks on the renewed left pectoral fin of an African Lepidosiren (*Protopterus annectens*) from a living specimen in the Society's Gardens.—Mr. Boulenger also exhibited young specimens and eggs of a South African Siluroid fish (*Galeichthys feliceps*), sent to him by Mr. J. M. Leslie, of Port Elizabeth. They had been taken from the mouth of the male parent, which carries its eggs in this extraordinary manner.—Prof. G. B. Howes read a paper on the probable existence of a Jacobson's organ among the Crocodilia, and made observations upon the skeleton of that organ in the Mammalia, and upon the basimandibular elements in the Vertebrata.—Mr. R. H. Burne made some observations on the variation and development of the Leporine sternum.—Mr. Scott B. Wilson read a paper on *Chasiempis*—a genus of Muscicapine birds peculiar to the Sandwich Islands. He described one of the species inhabiting the island of Oahu as new, and named it *Chasiempis gayi*, after Mr. F. Gay, of Kauai. The author further gave a key by which the five species of this genus inhabiting the various islands may be distinguished.—Mr. Wilson also read the description of a new bird of the genus *Himatione*—based on a single specimen obtained on the island of Maui—naming it *Himatione dolii*, after Mr. S. B. Dole, of Honolulu.—Mr. G. A. Boulenger read a paper on some British specimens of the remains of *Homœosaurus*, and made remarks on the classification of the Rhynchocephalia.—Mr. F. E. Beddard read a preliminary account of an Earthworm from West Africa, referable to a new genus and species, which he proposed to call *Libyodrilus violaceus*.—Mr. Frank Finn gave an account of a functional *ductus botalli* which he had observed in specimens of two birds (*Nycticorax violaceus* and *Dafila spinicauda*) dissected in the Society's Laboratory.

Royal Meteorological Society, February 18.—Dr. C. T. Williams, Vice-President, in the chair.—The following papers were read:—The great frost of 1890–91, by Mr. C. Harding. This paper dealt with the whole period of the frost from November 25 to January 22, and it was shown that over nearly the whole of the south-east of England the mean temperature for the 59 days was more than 2° below the freezing-point, whilst at seaside stations on the coast of Kent, Sussex, and Hampshire, the mean was only 32°. In the extreme north of Scotland as well as in the west of Ireland, the mean was 10° warmer than in the south-east of England. In the southern midlands and in parts of the south of England the mean temperature for the 59 days was more than 10° below the average, but in the north of England the deficiency did not amount to 5°, and in the extreme north of Scotland, it was less than 1°. The lowest authentic reading in the screen was 0°·6 at Stokesay, in Shropshire, but almost equally low temperatures occurred at other periods of the frost. At many places in the south and south-west of England as well as in parts of Scotland and Ireland, the greatest cold throughout the period occurred at the end of November; and at Waddon, in Surrey, the thermometer in the screen fell to 1°, a reading quite unprecedented at the close of the autumn. At Addington Hills, near Croydon, the shade thermometer was below the freezing-point each night, with one exception, and there were only two exceptions at Cambridge and Reading; whilst in the Shetlands there were only 9 nights with frost, although at Biarritz frost occurred on 31 nights, and at Rome on 6 nights. At many places in England the frost was continuous night and day for 25 days, but at coast stations in the north of Scotland it in no case lasted throughout the 24 hours. On the coast of Sussex the temperature of the sea was about 14° warmer than the air throughout December, but on the Yorkshire coast it was only 6° warmer, and in the Shetlands and on parts of the Irish coast it was only 3° warmer. The Thames water off Deptford, at 2 feet below the surface, was continuously below 34° from December 23 to January 23, a period of 32 days, whilst the river was blocked with ice during the greater part of this time. In Regent's Park, where skating continued uninterruptedly for 43 days, the ice attained the thickness of over 9 inches. The frost did not penetrate to the depth of 2 feet below the surface of the ground in any part of England, but in many places, especially in the south and east, the ground was frozen for several days at the depth of 1 foot, and at 6 inches it was frozen for upwards of a month. In the neighbourhood of London the cold was more prolonged that in any previous frost during the last 100 years, the next longest spell being 52 days in the winter of 1794–95, whilst in 1838 frost lasted for 50 days, and in 1788–89 for 49 days.—The problem of probable error as applied to meteorology, by Mr. T. W. Backhouse.

EDINBURGH.

Royal Society, February 2.—Sir Douglas Maclagan, President, in the chair.—Prof. W. Rutherford, by request of the Council of the Society, gave an address on the sense of hearing. He criticized Helmholtz's theory of the manner in which the cochlea is affected by sound-vibrations, and showed the great anatomical difficulties of any theory that regards the basilar membrane as the transmitter of sound-vibrations to Corti's cells, and as an analyst of complex sound-vibrations. The basilar membrane is heavily damped by the cellular elements above and beneath it; and, in the case of the rabbit, by its division into two layers with a homogeneous tissue between them, and also beneath the lower layer—an arrangement that must greatly interfere with any localized sympathetic vibration of its fibres. The hair cells of Corti are the true nerve terminations, and are placed in a favourable position for receiving the sound-waves transmitted to them through the superjacent membrane of Corti. The sound-wave is probably considerably damped, and fine vibrations therefore destroyed, by the granular protoplasm at the lower ends of Corti's cells and the granular cement substance between their lower ends and the supporting cells of Deiters. He stated the arguments opposed to the theory of Helmholtz, and explained his own theory of sound-sensation suggested by the telephone, and communicated by him to the British Association in 1885. Although no theory is free from difficulty, he maintained that the most feasible theory is that the great extension of the organ of Corti in the mammal is for the purpose of increasing the number of hair cells, so that the appreciation of qualities of sound may be more acute, and therefore more intelligent; that the hairs of all the cells of Corti are affected by tones of every pitch, and that the sound-vibrations are translated by Corti's cells into nerve-vibrations corresponding in frequency, amplitude, and form to those of the sound; and that the different sensations of tone are due to nerve-vibrations of different frequency and form, periodic or aperiodic, arriving in the sensorium. He illustrated his address by numerous diagrams and experiments, and was awarded at the close, on the motion of Sir William Turner, a special vote of thanks by the Society.

PARIS.

Academy of Sciences, February 23.—M. Duchartre in the chair.—M. Fremy, in presenting his recently published work on

the synthesis of rubies, remarked that he had been able to obtain numerous rhombohedric crystals identical with those found in nature. This result has been obtained, after many trials, by calcining a mixture of aluminium, red lead, and potassium bichromate for several hours in an earthenware crucible.—M. Chauveau presented a work having the title "Le Travail musculaire et l'énergie qu'il représente."—On coloured interference rings, by M. Mascart.—On the isolation of the glycolytic ferment of the blood, by MM. Lépine and Barral.—On the spectrum of α Lyræ, by M. H. Deslandres. Some photographs of the spectrum of α Lyræ were taken at Paris on the same dates as those taken by Mr. Fowler (*Monthly Notices R.A.S.*, December 1890). M. Deslandres's negatives, however, show K as a single, and not as a double line.—Observations of two new asteroids discovered at Nice Observatory on February 11 and 16, by M. Charlois. The magnitudes of the asteroids are 11·5 and 12 respectively.—Observations of the asteroid discovered by Charlois on February 11, made with the Brunner equatorial of Toulouse Observatory, by M. B. Baillaud. Observations for position were made on February 16 and 18.—Observations of sun-spots, made in 1889 and 1890 with the Brunner equatorial (0·18 metres aperture) of Lyons Observatory, by M. Em. Marchand. The following conclusions are deduced from the observations:—(1) The monthly numbers of groups did not vary much from January 1889 to January 1890; they increased from February 1890, this year presenting a total of 38 groups more than the previous one. (2) The total spotted surface per month varied little from January to August 1889; it then began to diminish, and passed a well-marked minimum in November of the same year. It afterwards increased more or less regularly up to the end of 1890, and in this year presented a total spotted surface of 103·3 thousandths of the area of the visible hemisphere, against 73·4 thousandths in 1889. (3) These facts place the minimum of solar activity in November 1889, a result in conformity with the absence of spots from October 10 to December 4 of the same year. (4) The distribution in latitude of the regions of activity changed completely about the time of minimum activity. Whilst at the beginning of 1889 spots were most frequent in the zone $-10°$ to $+10°$, in 1890 the maximum frequency occurred in the zones 20° to 30° in each hemisphere. What is more, the zones 30° to 40° north and south, in which only nine groups appeared in 1889, included thirty-two groups in 1890. (5) The southern hemisphere contained more active regions than the northern before the minimum (1889); the contrary was the case in 1890.—On the movement of a rectilinear vortex in a liquid contained in a rectangular prism of indefinite length, by M. Andrade.—On the representation of equations with four variables, by M. M. d'Ocagne.—On a class of harmonic surfaces, by M. L. Raffy.—On the compressibility of mixtures of air and hydrogen, by M. Ulysse Lala. The observations show that the compressibility of mixtures of air and hydrogen, in which the proportion of the latter gas varies from 16 to 31 per cent. is intermediate between those of air and hydrogen respectively for small pressures (about 100 cm. of mercury). With larger proportions of hydrogen and higher pressures the mixture appears to be less compressible than hydrogen itself. This interesting fact seems fully proved, for the experiments have been controlled by making a series of measures of the compressibility of single gases.—On the compression of quartz, by M. Monnory.—Direction of luminous vibrations; Fresnel's and Sarrau's systems, by M. E. Carvallo.—On the solubility of potassium bitartrate, by M. Ch. Blarez. It is shown that cream of tartar is completely insoluble at the ordinary temperature in a mixture of 100 parts of alcohol at 20°, 900 parts of water, 4 parts of neutral potassium sulphate, and 2 parts of tartaric acid, but is dissolved if all or part of the neutral sulphate is replaced by the acid sulphate.—On the transformation of the fecula in dextrine by butyric ferment (*Bacillus amylobacter*), by M. A. Villiers.—On normal butylamines, by M. A. Berg. The author has used Hoffmann's method for the preparation of these compounds by acting on normal chloride of butyl with an alcoholic solution of ammonia.—Determination of the form of parts of the sternum of vertebrate animals, by M. Lavocat.—The structure of the pancreas and intra-hepatic pancreas of fishes, by M. Laguesse.—Anatomy of *Cerianthus membranaceus*, by M. L. Faurot.—On the differentiation of the liber in the root, by M. P. Lesage.—On the native silver and the dioptase of the French Congo, by M. E. Jannettaz. Some mineralogical specimens brought from the Congo appear to contain native silver.—On the distribution of sea-salt according to altitude, by M. A. Muntz. According to analyses made by the author, the proportion of sodium chloride in the air decreases with the altitude. This result is easy to understand. Its importance is brought out by analyses of plants, rain-water, and the blood of different animals, which appear to vary in saltness in a similar manner.—M. G. Stefanesco announced the discovery of a manuscript containing an account of a fall of carbonaceous meteorites in 1774.

BRUSSELS.

Academy of Sciences, December 6, 1890.—M. Stas in the chair.—Facts demonstrating the permanence of the dark spots on Venus, and the slowness of their movement of rotation, by M. F. Terby. (See Our Astronomical Column.)—On some mollusks and post-Pliocene fossils found during a voyage up the Congo in 1887, by M. E. Dupont.—On the mollusks found by M. Dupont between the mouth of the Congo and the confluence of the Kassai, by M. Ph. Dautzenberg. The description of the specimens is accompanied by three plates.—Note on the physiology of Branchiostegans, by M. Léon Fredericq.—On the preservation of hæmo-cyanine when isolated from the action of air, by the same author.—Physical observations of the planet Mars in 1890, by M. J. Guillaume. (See Our Astronomical Column.)—On the influence of the exterior temperature on the production of heat in warm-blooded animals, by M. G. Ausiaux. M. Ausiaux has placed guinea-pigs in an Arsonval's calorimeter, submitted them to temperatures comprised between 3° and 32° C., and measured their production of heat for each degree. He finds that the minimum production of heat takes place at a temperature nearly equal to the mean diurnal temperature in spring, viz. 20° C. At temperatures above or below this the production of heat is increased.—New method for the quantitative determination of the quality of bread, flour, albumen, &c., by Dr. J. Barker Smith.

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